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The Long-run Effects of Teacher Strikes: *Evidence from Argentina*[a]

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Abstract

This is the first paper to estimate the effect of teacher strikes on student long-run educational attainment and labor market outcomes. We exploit cross-cohort variation in the prevalence of teacher strikes within and across provinces in Argentina in a difference-in-difference framework to examine how exposure to teacher strikes during primary school affects long-run outcomes. We find robust evidence that teacher strikes worsen the labor market outcomes of these individuals when they are between the ages of 30 and 40: being exposed to the average incidence of teacher strikes during primary school (88 days) reduces annual labor market earnings by 2.99 percent. A back-of-the-envelope calculation suggests that this amounts to an aggregate earnings loss of $712 million in Argentina annually. This is equivalent to the cost of raising the average annual employment income of all primary school teachers in Argentina by 19 percent. We also find evidence of a decline in hourly wage, an increase in unemployment, an increase in the probability of not working or studying and a decline in the skill levels of the occupations into which students sort. Examining short- and long-run educational outcomes suggests that the labor market effects are driven, at least in part, by a reduction in educational attainment. Our analysis further identifies significant intergenerational treatment effects. Children of adults who were exposed to teacher strikes during primary school also experience adverse educational attainment effects.

JEL-codes: I20, J24, J45, J52
Keywords: Strikes, Unions, Teachers, Education, Labor Market, Collective Bargaining, Public Policy

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1. Introduction

Teacher industrial action is a prevalent feature of public education systems across the globe; during the past few years teacher strikes have been observed in countries as diverse as Argentina, Brazil, Canada, Chile, China, Colombia, France, Germany, India, Israel, Italy, Lebanon, Mexico, Russia, Spain and the United States (e.g. Seattle, East St. Louis, Pasco, Prospect Heights and Chicago). A shared belief among policymakers across several of these countries is that teacher strikes are detrimental to student learning due to its negative effect on instructional time (Baker 2013). In some countries this sentiment has led to the enactment of legislation that severely restricts teachers' right to strike.[4] However, the effect of such restrictions on student outcomes is theoretically ambiguous because teacher strikes can also result in better working conditions that motivate teachers and raise their productivity. Despite this theoretical ambiguity, there is a lack of empirical research that credibly and comprehensively evaluates how teacher strikes affect student outcomes.

In this paper, we construct a new data set on teacher strikes in Argentina and use this to present the first evidence in the literature on the long-run educational attainment and labor market effects of teacher strikes. Between 1983 and 2014 Argentina experienced a total of 1,500 teacher strikes, with substantial variation across time and provinces, making this an interesting case for the study of teacher strikes. We analyze the relationship between exposure to strikes during primary school and relevant education, labor market and other socioeconomic outcomes when the affected cohorts are between 30 and 40 years old.[5] We also investigate if the effects that we estimate carry over to these individuals' children.

To identify the effect of teacher strikes, we rely on a cross-cohort difference-in-difference method that examines how education and labor market outcomes changed among adults who were exposed to more days of teacher strikes during primary school compared to adults who were exposed to fewer days of teacher strikes during primary school. The sources of variation we exploit therefore come from within-province differences in strike exposure across birth cohorts and within-cohort differences in strike exposure across provinces.

The main identifying assumptions underlying our estimation strategy are that there are no shocks (or other policies) contemporaneous with teacher strikes that differentially affect

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[4] For example, even though 33 states in the US have passed duty-to-bargain laws that require districts to negotiate with a union (if teachers have elected one for the purpose of collective bargaining), only 13 states allow teachers to go on strike in the event of a bargaining impasse (Colasanti 2008).

[5] We focus on this age range because existing literature suggests that labor market outcomes at this age are informative about lifetime labor market outcomes (e.g. Haider and Solon 2006)

the various cohorts and that the timing of teacher strikes is uncorrelated with prior trends in outcomes across birth cohorts within each province. We show extensive evidence that our data are consistent with these assumptions. In particular, our results are robust to controlling for local labor market conditions, including province-specific linear time trends, accounting for cross-province mobility, excluding regions with persistently high frequencies of teacher strikes, and controlling for province-specific non-teacher strikes. This suggests that our estimates are not driven by province-specific variation in macroeconomic performance across time and that there are no shocks contemporaneous with teacher strikes that differentially affect the various cohorts. We also show that the effects we identify disappear when reassigning treatment to cohorts that have just graduated from primary school, indicating that the timing of teacher strikes is uncorrelated with prior trends in outcomes across birth cohorts within each province.

We find robust evidence that teacher strikes worsen future labor market outcomes: being exposed to the average incidence of teacher strikes during primary school (88 days) reduces annual labor market earnings and wages for 30-40 year olds by 2.99 percent and 2.22 percent, respectively. Based on these results, the implied rate of return to an additional year of primary education in Argentina (180 days) is 6.1 percent. The prevalence of teacher strikes in Argentina means that the effect on the economy as a whole is substantial: A back-of-the-envelope calculation suggests an aggregate annual earnings loss of $712 million. This is equivalent to the cost of raising the average employment income of all primary school teachers in Argentina by 19 percent.

In addition to adverse wage and earnings effects, our results reveal negative effects of teacher strikes on several other education and labor market dimensions as well. Specifically, our results indicate that being exposed to the average incidence of teacher strikes during primary school leads to a 0.70 of a percentage point increase in unemployment (11.44 percent relative to the mean) and a 1.58 percentage point increase in the probability of not working or studying (7.92 percent relative to the mean). We also find evidence that teacher strikes causes individuals to sort into lower-skilled occupations later in life. Examining short- and long-run educational outcomes demonstrate that these adverse labor market effects are driven, at least in part, by a reduction in educational attainment: being exposed to the average incidence of teacher strikes during primary school leads to a reduction in years of education by 1.84 percent relative to the mean. Finally, we document significant intergenerational treatment effects: children of individuals exposed to teacher strikes during primary school suffer negative educational attainment effects as well.

Our results further demonstrate that teacher strikes affect men and women very differently. For males, exposure to teacher strikes leads to a reduction in educational attainment, an increase in the likelihood of being unemployed, occupational downgrading, and has adverse effects on earnings as well as wages. For females, teacher strikes reduce educational attainment in a way similar to that of men. We find a reduction in the level of earnings among females as well, but teacher strikes do not affect the wages of females who are employed. We show that this is because teacher strikes induce females to sort into home production (defined as neither working nor studying). Our analysis reveals that teacher strikes affect women on several additional socioeconomic dimensions as well. Specifically, females exposed to teacher strikes during primary school have more children, less educated partners, and lower per capita family income. We argue that some of these effects are driven by a decline in female's bargain power within the household.

Our paper contributes to the existing literature in several important ways. First, no other paper has examined the effects of teacher strikes on student long-run outcomes. Given the large literature demonstrating that short-run program effects on student outcomes can be very different from any effects on long-run outcomes, this is of great value to policy makers (e.g., Chetty et al. 2011; Deming et al. 2013; Lovenheim and Willén 2016). Second, the frequency and prevalence of teacher strikes that we exploit is much greater than that which has been used in earlier studies. This allows us to obtain more precise estimates, and examine a richer set of outcomes, compared to what has been done before. Third, this paper makes use of a novel data set which we have created based on information from annual business reports on the Argentine economy. This data is a great tool for other researchers interested in questions centering on teacher strikes and industrial action.

It is important to highlight that the pervasive level of teacher strikes during our analysis period is not a deviation from the norm in Argentina, and current student cohorts are exposed to similar levels of strikes. This cements the relevance of our paper and highlights the urgency of implementing reforms that reduce the prevalence of teacher strikes in the country. One policy could be to introduce labor contracts that extend over several years, and only allow teachers to strike if a bargaining impasse is reached when renewing these multi-year contracts. This would eliminate sporadic teacher strikes while still allowing teachers to use industrial action as a tool to ensure fair contracts.

The rest of this paper is organized as follows: Section 2 provides an overview of the education system in Argentina and offers theoretical predictions of how teacher strikes may affect student outcomes; Section 3 discusses pre-existing research; Section 4 introduces the data; Section 5 presents our empirical strategy; Section 6 discusses our results; and Section 7 concludes.

2. Background & Theoretical Predictions of Teacher Strikes

2.1 The Argentinian Education System

Education in Argentina is the responsibility of the provinces and consists of four levels: kindergarten, primary education, secondary education and tertiary education.[6] Primary education begins the calendar year in which the number of days the child is 6 years old is maximized, and comprises the first seven years of schooling. Prior to the implementation of the *Federal Education Law* in 1998 (approved in 1993), only primary education was mandatory in Argentina (Alzúa et al. 2015). Since then, compulsory schooling has grown to include secondary education as well, increasing the length of mandatory education from 7 to 12 years. Public education is financed through a revenue-sharing system between the provinces and the federal government which is funded by taxpayers, and is free at all levels.

The fraction of students that attended private school at the primary level during our analysis period was approximately 0.2, and this fraction was held relatively constant across the years that we examine. Since 2003, however, private enrollment at the primary and secondary level has increased substantially. Existing research suggests that this increase is driven by high- and middle-income families, leading to an increase in socioeconomic school segregation (Gasparini et al. 2011; Jaume 2013).[7]

2.2 Teacher Strikes in Argentina

The presence of unions, collective bargaining and labor strikes in Argentina can be traced back to the early years of the 20th century, except for the years during which the country was subject to military dictatorships (Confederacion de Educadores Argentinos 2009). During the dictatorships (the most recent one lasting from 1976 to 1983), labor strikes were prohibited and collective bargaining limited. Following the reinstatement of democracy in 1983, industrial action has quickly regained its status as a pervasive feature of the Argentine labor

[6] Primary education was decentralized in 1978 and secondary education was decentralized in 1992. However, the national government remains highly involved in terms of setting curriculum, regulations and financing.
[7] A commonly held belief is that individuals perceive private education as superior due to the fact that teacher strikes are much less pronounced at private institutions, but existing literature finds no effect of teacher strikes on the likelihood of being enrolled at a public institution (Narodowski and Moschetti 2015). We examine this in detail in Section 6.4.

market. Since then, public sector teachers have been the most active social protesters in the country, and current estimates suggest that they make up approximately 35 percent of all labor strikes in Argentina (Chiappe 2011; Etchemendy 2013). In comparison, private school teachers account for less than 4 percent of all labor strikes in the country. The occupation with the second largest incidence of labor strikes in modern times is public administration, accounting for approximately 25 percent of all strikes (Chiappe 2011; Etchemendy 2013).

Teacher unions are typically organized at the provincial level, and variation in teacher strikes across time and provinces is substantial. On average, provinces have lost 372 instructional days due to teacher strikes between 1983 and 2014 (6.7 percent of total instructional days), ranging from 188 days (3.3 percent) in La Pampa to 531 days (9.5 percent) in Rio Negro, with a standard deviation of 109 days.[8] The pervasive level of teacher strikes during our analysis period is not a deviation from the norm in Argentina, and current students are exposed to similar levels of teacher industrial action. This highlights the importance and relevance of our results. Figure 1 shows the variation in the number of days of teacher strikes by province during the period 1977 and 2014, and Figure 2 displays the the number of strikes by province during the same period (a strike can last for a couple of hours or for several weeks).

Although there is no existing research that investigates the effect of teacher strikes on student outcomes in Argentina, several studies have attempted to disentangle the factors underlying the prevalence of teacher strikes in the country. The results are mixed: Murillo and Ronconi (2004) finds that teacher strikes are more common in provinces where union density is high and political relations with the local government is tense, while Narodowski and Moschetti (2015) concludes that days of teacher strikes display an erratic behavior without any discernable trends or explanations. What these two studies have in common is that they both emphasize the lack of a relationship between local labor market conditions and teacher strikes. This result is important for our empirical strategy since our main identification assumption is that there are no shocks contemporaneous with teacher strikes that differentially affect the different cohorts (this assumption is explored in detail in Section 6.3).

In summary, this section first described the prevalence of teacher strikes in Argentina since 1983. It then showed that there is substantial variation in teacher strikes across provinces in any given year and across years in any given province. Finally, it pointed to prior

[8] There are 180 instructional days per year in Argentina. The total number of instructional days between 1983 and 2014 is therefore 5,760. 372 out of 5,760 is 0.067, or 6.7 percent.

findings in the literature that indicates that teacher strikes in Argentina likely are not driven by local labor market conditions.

2.3 Theoretical Predictions

The main way in which teacher strikes can affect student outcomes is by reducing the time students spend in school. Theoretical as well as empirical research of education production provide clear predictions about the consequences of reduced instructional time: lower academic achievement (Cahan and David 1987; Cahan and Cohen 1989; Neal and Johnson 1996; Lee and Barro 2001; Gormley and Gayer 2005; Cascio and Lewis 2006; Luyten 2006; Pischke 2007; Marcotte 2007; Sims 2008; Marcotte and Helmet 2008; Hansen 2008; Leuven et al. 2010; Fitzpatrick et al. 2011; Rivkin and Schiman 2013; Goodman 2014). However, teacher strikes may not only affect student outcomes through lost instructional time, and it would be incorrect to attempt to predict the likely consequences of teacher strikes by solely referencing these studies.[9]

In addition to reducing effective instructional time, teacher strikes can (1) affect teacher effort, (2) alter resource levels and allocation, (3) affect academic expectations and graduation requirements, (5) alter the value of a diploma, (6) change the value differential between a public and a private degree, and (7) change the composition of teachers. The direction and magnitude of the effects flowing through these different channels will depend on the nature and outcome of the strike. For example, if the unions go on strike to raise wages and are successful, the strike will likely lead to an increase in teacher effort and productivity. This could also lead to an improvement in the composition of the teacher workforce in the long-run.[10] However, if the strike is in effect for several months before the two sides reach an agreement, academic expectations and graduation requirements may be adjusted downwards with the potential implications of a reduction in the value of a diploma and an increase in the value differential between a public and a private degree. Further, the increase in teacher pay may be financed through a reallocation of resources from other inputs that enter the education production function, and this can lead to a reduction in educational quality. The effect of

[9] Many of the predictions of the effects of teacher strikes are related to the underlying reasons for teachers to strike. It is therefore difficult to determine the generalizability of our results to other countries and settings, as teachers in for example the US may strike for other reasons than those that lead teachers in Argentina to strike. In a companion paper with Gustavo Torrents (Indiana University), we build a political economy model that aspires to identify the most common drivers of teacher strikes in Argentina. The outcome of that paper should be used to determine the generalizability of the results in the current paper to other countries and settings.

[10] This would take time and highlights the importance of analyzing long-run effects.

teacher strikes on education production can thus be both positive and negative. The resulting predictions of the effects of teacher industrial action on student outcomes are therefore ambiguous.

Two additional factors augment this theoretical ambiguity. First, there may be substantial treatment heterogeneity across students. The most likely source of heterogeneity concerns the socioeconomic characteristics of the students' families: wealthy parents will be able to move their children to private institutions if they believe the strikes to hurt their children. If this behavior is sufficiently pervasive it may lead to a segregated school system with additional adverse effects on the students from poor families that are left behind. This effect may be further augmented if teachers from poorer districts are more likely to join teacher unions and participate in strikes. Another source of treatment heterogeneity relates to when during primary school children are exposed to strikes. Ample research suggests that younger children are more susceptible to policy interventions in general, and children who lose several weeks of instructional time in first grade may therefore suffer more than children who lose the same amount of days in the final grade of primary school (Shonkoff and Meisels 2000; Cunha and Heckman 2007; Doyle et al. 2009; Chetty et al. 2015).

Second, teacher strikes may have important effects on non-educational outcomes. The reason is that teacher strikes reduce effective instructional time. Unless parents can make alternative educational arrangements (which will depend on whether it was an expected or unexpected strike, and on the resources that the parents possess), this will lead to an increase in leisure time and to an increase in the risk of engaging in bad behavior and criminal activity (e.g. Anderson 2014; Henry et al. 1999). This can directly impact the future education and labor market outcomes of children.

The above discussion demonstrates that teacher strikes reduce instructional time, and existing models make clear that reductions in instructional time negatively impact student learning. However, the discussion also makes clear that strikes can affect students through a number of other channels, and that the magnitude and direction of those effects depend on the cause and outcome of the strike. In addition, there may be substantial treatment heterogeneity associated with teacher strikes. Therefore, the net effect of teacher strikes on long-run educational attainment and labor market outcomes is ambiguous. This underscores the importance of the empirical analysis presented here.

3. Prior Literature on Teacher Strikes

The majority of the existing research on teacher strikes is cross sectional with identification strategies that are vulnerable to omitted variable bias (Caldwell and Maskalski 1981; Caldwell and Jefferys 1983; Zirkel 1992 Thornicroft 1994; Zwerling 2008; Johnson 2009). Specifically, students, teachers and schools subject to strikes may be systematically different from those that are not exposed to strikes on dimensions that we cannot observe. If these differences have independent effects on the outcomes that are being examined, this will confound the estimated effect of teacher strikes on outcomes. Further, these studies have focused exclusively on contemporaneous education effects (test scores) of teacher strikes that are of very short duration. These two factors significantly limit our understanding of the consequences associated with teacher industrial action. This is particularly the case given the large literature suggesting that short-run program effects on student outcomes can be different from any effects on long-run outcomes (e.g., Chetty et al. 2011; Deming et al. 2013; Lovenheim and Willén 2016).

Abstracting away from potential identification issues, the results from the above studies are mixed. While some studies find no association between strikes and student outcomes (e.g. Zwerling 2008; Thornicroft 1994; Zirkel 1992), others find marginally statistically significant and negative effects (e.g. Johnson 2009; Caldwell and Maskalski 1981 and Caldwell and Jefferys 1983). Taken together, these studies suggest that the anti-strike bans imposed in numerous countries across the globe are marginally justified at best.

To the best of our knowledge, only two studies that look at the effect of teacher strikes on student outcomes have relied on research designs that are not cross sectional: Belot and Webbink (2010) and Baker (2013). Belot and Webbink (2010) exploit an institutional reform in Belgium in 1990 that led to substantial and frequent strikes in the French-speaking community but not in the Flemish-speaking community of the country. Using a difference-in-difference approach that compares the difference in educational outcomes between individuals in school to those not in school in the French-speaking community to that same difference in the Flemish-speaking community, the authors find some evidence in favor of teacher strikes causing a reduction in educational attainment and an increase in class repetition. Though interesting, this study is not able to examine if the identified education effects carry over to the labor market, if there are other non-educational effects of teacher strikes or if there are intergenerational treatment effects. Further, the point estimates in Belot and Webbink (2010) provide the intent-to-treat effect of exposure to all strikes in 1990 among students in all grade school years. This makes it difficult to extrapolate the marginal effect of teacher strikes on students in specific school grade years.

Baker (2013) evaluates the effect of teacher strikes on student achievement in Ontario by comparing the change in test score between grade 3 and 6 for cohorts exposed to a strike to the corresponding change for cohorts that were not subject to a strike. The results suggest that strikes that lasted for more than 10 days and took place in grade 5 or 6 have statistically and economically significant negative effects on test score growth, while strikes that occurred in grades 2 or 3 do not have statistically or economically significant effects. The research design used by Baker (2013) is less exposed to omitted variable problems than the abovementioned studies as it allows the author to control for unobserved factors provided that they are fixed at the school or student cohort level. However, data limitations prevent the author from examining long-run educational attainment and labor market effects – one of the main contributions of the current analysis.

To summarize, the majority of the existing research on teacher strikes is cross sectional with identification strategies that are vulnerable to omitted variable bias. More current papers rely on identification strategies less susceptible to such econometric issues, but limited variation in teacher strikes coupled with lack of good outcome data has led these studies to only examine the educational effects of teacher strikes in the short- and medium-term. There is no existing research that has explored the long-run educational attainment and labor market effects of exposure to teacher strikes. Further, no study has been able to examine if there are intergenerational treatment effects associated with teacher strike exposure, and no existing analysis has examined potential nonlinear and heterogeneous treatment effects of teacher strikes. These gaps in the literature prevent us from fully understanding the dynamics of teacher industrial action, and whether the net effect of such policies is beneficial or harmful to students. This cements the importance of our empirical investigation on the topic.

4. Data

4.1 Teacher Strikes

Data on teacher strikes by province and year are obtained from the annual reports on the Argentine economy published by Consejo Técnico de Inversiones (CTI). These annual reports provide province- and sector-specific information on labor strikes (duration and number of workers) per month, and we use information from 1977 to 1998 to construct our data set. We assume that children begin school the calendar year they turn 6, and graduate from primary school at the age of 12. This means that we have information on exposure to teacher strikes while in primary school for children born between 1971 and 1985. The assumption that children attend primary school between the ages of 6 and 12 leads to some measurement error in treatment assignment because children start primary school the calendar year in which the number of days they are 6 years old is maximized.[11] This assumption will thus cause a slight attenuation of our results. Using household survey data on the educational attainment of 6 year olds between 2003-2015, we estimate that 70 percent of individuals in our sample are assigned to the right cohort.

We restrict our analysis to strike exposure during primary school, rather than during primary and secondary school, for two reasons. First, our data shows that the fraction of individuals that completed secondary education during our analysis period was less than 0.6. If we include strike exposure during secondary school this means that we would assign the wrong treatment to more than 40 percent of the sample (as our analysis is based on aggregated birth year – birth province data). This would introduce an attenuation bias that makes the results difficult to interpret. Second, institutional features of the Argentinian education system make strikes less common at the secondary level, and while all strikes reported by CTI affect primary school teachers, only a fraction of them affect secondary school teachers. We cannot identify which fraction of the CTI-reported strikes that are relevant to secondary school teachers, and the treatment variable would therefore be very noisy at this level.

Table 1 displays the cross-cohort variation in exposure to teacher strikes within and across provinces that we use as identifying variation. The table shows that there is substantial variation both within provinces over time and across provinces in any given year. Table 1 also shows that the average number of days of teacher strikes that these cohorts were exposed to during primary school is 40 (or 3.2 percent of primary school instructional days).[12] If one takes national teacher strikes into account this number increases to 88 (or 6.98 percent of primary school instructional days).[13] As discussed in Section 2, strikes were prohibited during the military junta of 1977-1983. This explains why the oldest cohorts in our sample are exposed to relatively fewer days of teacher strikes.

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[11] To precisely impute the number of strikes during primary schooling we would need information on the month and day that each child was born on, which is not available in the survey.

[12] Primary school in Argentina is comprised of 1260 instructional days, 180 days per year.

[13] We do not consider national teacher strikes when constructing our treatment measure as they are completely subsumed by the cohort fixed effects that we use. See Section 5.

4.2 Educational Attainment & Labor Market Outcomes

Our outcome data come from the 2003-2015 waves of the Encuesta Permanente de Hogares (EPH), a household survey representative of the urban population of Argentina (91 percent of the total population). Our main analysis focuses on individuals between the ages of 30 and 40 because these individuals have typically completed their education and are on a part of their earnings profile where their earnings are reflective of lifetime earnings (e.g. Haider and Solon 2006; Böhlmark and Lindquist 2006). Table 2 shows the birth cohort that underlies each year and age combination that we use for our analysis and Figure 2 provides a visual depiction of the data structure for a subsample of birth cohorts. As shown in Table 2, the birth cohorts range from 1971 to 1985. These are the only cohorts that are between 30 and 40 years old when the outcomes are measured (2003-2015) for which we can perfectly calculate exposure to teacher strikes during primary school. This means that we do not have a balanced panel of age observations across the EPH waves. In Section 6.3 we show that limiting our analysis to EPH waves 2011-2015 for which we have a balanced panel has no impact on our results.

Crucial to our identification strategy is our ability to link respondents to their province of birth, because teacher strikes may lead to selective sorting across provinces, especially if exposure to strikes affects school quality. Teacher strikes could also impact post-primary school mobility patterns if strike-induced education effects affect one's access to national labor markets. Relying on birth province rather than current province of residence eliminates these endogenous migration issues. It is still the case that a fraction of respondents will be assigned the wrong treatment dose as families can move across provinces such that birth province is different from the province in which the child attended primary education. However, Table 3 shows that the province of residence is the same as the birth province for 93 percent of 13 year olds in Argentina, and any bias resulting from this mobility is therefore likely to be very small. In Section 6.3 we further show that our results are robust to excluding the five provinces with the highest migration rates.

To construct our analysis sample, we collapse the data on the birth province – birth year – EPH year level. Aggregation to this level is sensible because treatment varies on the birth province – birth year level. Table 4 provides summary statistics of the outcome variables we use in our analysis. For educational attainment, we generate dummy variables for completion of secondary education and for having obtained at least a Bachelor's degree. These indicators are constructed from a years of education variable that we also use to examine the educational attainment effect of strike exposure. With respect to labor market outcomes, we look at the proportion of people that are unemployed, out of the labor force and dedicated to home production (neither studying nor working). To construct a measure of occupational skill we follow Lovenheim and Willén (2016) and calculate the fraction of workers in each 3-digit occupation code that has more than a high school degree. We use this to rank occupations by skill level to examine if strike exposure leads individuals to sort into lower-skilled occupations.[14] We also use the EPH measures of hours worked and earnings. With respect to earnings, we consider both the log of hourly wage and log of total labor earnings. Since teacher strikes may affect labor force participation and unemployment, we also study the effect on the level of total labor earnings, which includes individuals with zero earnings.

Preliminary evidence on the relationship between teacher strikes and student long-run outcomes is displayed in Figure 3, which plots the predicted years of schooling (Panel A) and labor earnings (Panel B) as a function of the number of days of teacher strikes during primary school.[15] Across the panels, there is clear suggestive evidence of a strong linear negative correlation between exposure to teacher strikes and later-in-life outcomes: For each 180 days of teacher strikes (equivalent to a full year of primary school) labor earnings are reduced by 6.7 percent, and years of education declines by 3.1 percent, relative to the sample means.[16] Even though the descriptive evidence in Figure 3 is instructive, it is important to note that causal inference cannot be made from these graphs.

In addition to the education and labor market outcomes discussed above, we examine the effect of strike exposure on several socioeconomic and demographic outcomes: the likelihood of being the household head or spouse to the household head; the likelihood of being married; the number of children in the household; the age of the oldest child; the education level of the partner; and the per capita income of the household. We also analyze intergenerational effects by examining the effect of teacher strikes on two educational outcomes of children to individuals who were exposed to strikes in primary school. We first construct a dummy variable that equals 1 if the child is not delayed at school (age of the child minus years of education plus 6 is greater than zero). We then construct a variable of the educational gap of the child, defined by years of schooling plus 6 minus age. We collapse these variables at the household level.

[14] We also construct two alternative measures of teacher skill based on average years of education and average wage in the occupation. The results are robust to these alternative measures.
[15] These results are produced by a model that includes birth year, birth province and calendar year fixed effects. See the figure notes for detailed information.
[16] 180 days is also the difference between the 10th and the 90th percentile of teacher strike exposure among the individuals included in our sample.

4.3 Local Labor Market Controls

One of the main threats to our research design is the possibility that teacher strikes are driven by local labor market conditions such that the effects we identify do not represent the effect of exposure to teacher strikes during primary school holding all else constant, but rather the effect of teacher strikes *and* local labor market conditions during primary school.

To minimize this threat to identification we include two variables in our estimating equation that serve to control for variation in local labor market conditions across provinces and time. First, we collect data on public administration strikes by province and year from CTI (the occupation with the largest number of strikes during our analysis period after teachers) and compute days of exposure to public administration strikes for each birth year - birth province cell during primary school.[17] By controlling for public administration strikes, we exploit variation in teacher strikes net of any general province-specific events and conditions that fuel labor conflict. Second, we collect data on province-specific Gross Domestic Product (GDP). This data comes from Mirabella (2002), who estimates province GDP using residential electricity consumption. We average the province-specific GDP during the seven years of primary school for each birth year -birth province cell.

The inclusion of these controls significantly reduces the risk that our point estimates are driven by local labor market conditions; such local labor market conditions would have to be uncorrelated with province GDP and public administration strikes but correlated with teacher strikes and have an independent effect on the outcomes that we examine.

5. Empirical Methodology

We exploit cross-cohort variation in exposure to teacher strikes during primary school within and across provinces in a difference-in-difference framework. Specifically, we estimate models of the following form:

$$Y_{pct} = \beta_0 + \beta_1 TS\_Exposure_{pc} + \gamma X_{pc} + \emptyset_t + \vartheta_c + \varphi_p + \delta T_c + \theta T_p + \varepsilon_{pct} \qquad (1)$$

where Y_{pct} is one of the education or labor market outcomes listed above for respondents born in province *p*, in birth cohort *c* and observed in EPH calendar year *t*. Regressions are weighted

[17] Public administration strikes make up more than 25 percent of all labor strikes in Argentina (Chiappe 2011; Etchemendy 2013).

by the number of observations in each birth province - birth year - calendar year cell.[18] The treatment variable of interest is $TS\_Exposure$ and measures the number of days (in tens of days) that the cohort was exposed to teacher strikes during primary school.[19]

Equation (1) also includes province (φ_p), birth cohort (ϑ_c) and calendar year ($\emptyset_t$) fixed effects as well as a province-specific linear time trend (θT_p) and a cohort-specific linear time trend (δT_c). The province-specific linear time trend absorbs any trend in Y over time within a province, and the cohort-specific linear time trend absorbs any trend in outcomes over time within a birth cohort. Equation (1) further contains a vector of province-specific covariates (X_{pc}) that control for average socioeconomic and demographic characteristics of the province while the cohort was in primary school.[20]

In addition to using equation (1) as defined above, we also estimate versions of the model that substitute the linear time trends for birth province-by-calendar year and birth year-by-calendar year fixed effects. The province-by-calendar year fixed effects control for variation in Y that is common across birth cohorts within a province in a given year (e.g. province-specific macroeconomic shocks) and the birth year-by-calendar year fixed effects control for any systematic difference across birth years that may be correlated with exposure to teacher strikes and the outcomes of interest. Though this model is more flexible than equation (1), it is a very demanding specification, in particular bearing in mind the relatively low number of observations that we use in our main analysis. Because the results produced by this model are not statistically significantly different from those obtained from estimation of equation (1), we consider equation (1) to be our preferred specification.[21]

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[18] Standard errors are clustered on the birth province – birth year level. The results are robust to clustering at the birth province only, but due to the small number of provinces we prefer the two-way clustering option.

[19] It is possible that teacher strikes have non-linear effects on educational attainment and labor market outcomes, such that the first ten days of strikes is more harmful to students than the next ten days. This could be because it takes time for parents to make alternative education arrangements for their children, such that the first days of a teacher strikes are more damaging. We have investigated this possibility by adding a quadratic term of our treatment variable in the estimation of equation (1). Though we do find some evidence in favor of the effect of strike exposure being larger for the first days of strikes for a few outcomes among males, we fail to identify a consistent pattern. Results are available upon request.

[20] In results not shown, we have also estimated this equation using number of strikes, rather than number of days of strikes, as our measure of treatment intensity. The results obtained from this alternative specification are consistent with the results presented in this paper: the number of strikes exposed to during primary school is associated with negative educational attainment and labor market effects. We further find substantial heterogeneity when using this alternative measure: the negative effects are driven exclusively by strikes that lasted for more than two days. That the effects are dependent on the length of the strikes is consistent with Baker (2013).

[21] We also perform our analysis using an instrumental variable approach in which we instrument teacher strikes with public administration strikes. This estimation strategy relies on a set of assumptions that are distinct from our preferred cross-cohort difference-in-difference method: that exposure to public administration strikes must be a good predictor of exposure to teacher strikes and that, conditional on the covariates and fixed effects included in the model, exposure to public administration strikes cannot have an independent effect on the outcomes of interest. The most serious threat to the exclusion restriction is that public administration strikes may have an effect on student outcomes that does not operate through exposure to teacher strikes (which is why we have included exposure to public administration strikes as a control variable in equation (1)). However, given the rich set of fixed effects as well as the control for province-specific GDP that we include in

The unit of observation is a birth province – birth year – calendar year, and the identifying variation stems from cross-cohort variation in exposure to teacher strikes during primary school within and across provinces. There are two main identifying assumptions underlying our estimation strategy. First, that there are no shocks (or other policies) contemporaneous with teacher strikes that differentially affect the different cohorts. The most serious threat to this identification assumption is that teacher strikes may be a reflection of political events, economic conditions or social situations that also vary at the birth province – birth year level and independently affect the outcomes of interest. This would confound our results and lead to invalid inference. To explore this possibility we incorporate the number of days (in tens of days) that the cohort was exposed to public administration strikes during primary school as an additional control variable in equation (1). We further control for average province-specific GDP during primary school to ensure that our results are not driven by local booms and busts that may be correlated with teacher strikes.

Controlling for province-specific GDP and public administration strikes significantly reduces the risk that our point estimates are driven by local labor market conditions or secular shocks; such shocks would have to be uncorrelated with provincial GDP and public administration strikes but correlated with teacher strikes and have an independent effect on the outcomes that we examine (and survive the inclusion of fixed effects and linear time trends). Further, to the best of our knowledge, there are no other relevant policies that occurred concurrently with these teacher strikes that are correlated both with variation in teacher strikes across provinces and the outcomes that we examine.

The second assumption underlying our estimation strategy is that the timing of teacher strikes must be uncorrelated with prior trends in outcomes across birth cohorts within each province. The conventional method for examining the validity of this assumption is to estimate event-study models that non-parametrically trace out pre-treatment relative trends as well as time varying treatment effects. Our research design does not lend itself well to this approach, and we rely on two alternative methods for illustrating that the timing of teacher strikes is uncorrelated with prior trends in outcomes across birth cohorts within each province.

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our model, this is unlikely. Our main results are robust to this alternative approach. The main take-away from this exercise is that – even if we cannot ascertain the validity of the assumptions underlying either one of our two estimation methods – the fact that our results are insensitive to which of these methods we use significantly limit the sources of bias that can invalidate our results. The reason is that the two methods rely on completely different sets of assumptions. Results from the instrumental variable approach are available upon request.

First, we incorporate province-specific linear time trends to show that our results are not driven by trends in outcomes across birth cohorts within each province. Second, we reassign the treatment variable for birth cohort *c* to birth cohort *c-7*, such that the measure of exposure to teacher strikes is the number of days (in tens of days) of primary school strikes that took place while the individuals were 13 – 19 years old. As these individuals have already completed primary school they should be unaffected by these strikes, and the coefficient on *TS_Exposure* should not be statistically or economically significant.[22]

6. Results

6.1 Labor Market & Education Effects of Teacher Strikes

i. Educational attainment

Table 5 presents baseline estimates of the effect of teacher strikes on educational attainment for the full sample (Panel A) as well as for males (Panel B) and females (Panel C) separately. Each cell in the table comes from a separate estimation of equation (1), and we add controls sequentially across columns. In Column 1, we control for birth province, birth year and EPH survey year fixed effects as well as local GDP and exposure to public administration strikes. We add a cohort-specific linear time trend and a province-specific linear time trend in Column 2. In Column 3, we replace the linear time trends from Column 2 with birth province-by-EPH survey year and birth year-by-EPH survey year fixed effects.

The estimates in Table 5 provide clear evidence of a negative effect of teacher strikes on educational attainment. The sequential addition of controls across the columns does not have a statistically or economically significant effect on the point estimates. As elaborated on in Section 5, the model underlying the estimates in Column (2) is our preferred specification.[24] We base the majority of the discussion of our results on this model.

The estimates in Panel A indicate that being exposed to teacher strikes for ten days during primary school (0.79 percent of total time in primary school) reduces the proportion of people in the birth year – birth province cell that obtain a high school diploma by 0.0028,

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[22] It should be noted that 13-19 year olds were exposed to teacher strikes as well. To the extent that teacher strikes are correlated across years within provinces, this model may produce economically and statistically significant results. This makes any null results obtained through this falsification test even more powerful in terms of supporting our identifying assumptions.

[24] While the model used to obtain the estimates displayed in Column (3) is more flexible, it is very demanding, in particular bearing in mind our relatively small sample of 4,032 birth province – birth year – EPH survey year observations. Further, the estimates in Column (2) are not statistically significantly different from those in Column (3).

lowers the proportion that receive a college degree by 0.0015 and reduces the number of years of education by 0.024. This suggests that ten days of exposure to teacher strikes during primary school increases the number of people that do not graduate from high school by 28 out of every 1,000 and increases the number of people that do not finish tertiary education by 15 out of every 1,000. These effects represent declines of 0.48 percent, 0.68 percent and 0.21 percent relative to the respective means, which is shown directly below the estimates in the table. A comparison of Panel B and Panel C reveals that males are more affected by teacher strikes, though the effects are statistically and economically significant among individuals of both genders. That the effects are stronger for men is consistent with the large literature that shows boys to be more sensitive than girls to educational interventions and adverse shocks during childhood (Krueger 1999; Autor and Wasserman 2013; Bertrand and Pan 2013; Fan et al. 2015; Lovenheim and Willén 2016; Autor et al. 2016).

The average individual in our sample experienced a total of 88 days of teacher strikes during primary school. Scaling the point estimates to account for the mean level of exposure (multiplying the point estimates by 8.8) suggests that the average cohort in our sample suffered adverse educational attainment effects with respect to the proportion of people obtaining a high school diploma, a college degree and years of education equivalent to 4.18, 6.38 and 1.84 percent respectively, relative to the means.[25]

Taken together, the results in Table 5 suggest that exposure to teacher strikes not only has adverse short-term educational attainment effects (as measured by the reduction in the proportion that obtain a high school diploma), but that these effects persist as individuals move through the various stages of the education system (as measured by the proportion that obtain a college degree and the average number of years of education).[26] This is an important finding that has not been documented before. The results show that a teacher's decision to strike results in permanent harm to his or her students' average educational outcomes.

ii. Employment, labor force participation & home production

Existing economics of education research has documented a strong positive relationship between educational attainment and later-in-life labor market opportunities (e.g. Ashenfelter

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[25] This rescaling assumes linear treatment effects. Given the suggestive evidence in Figure 3 this is not an unreasonable assumption. Further, when we relax this assumption in Section 6.2 our results do not change.

[26] In section 6.4 we study the effect of teacher strikes on contemporaneous educational outcomes for children aged 12-17, something that we cannot do for our main analysis sample due to data limitations. This auxiliary analysis reveals negative educational effects consistent with the results for older cohorts discussed in this section.

et al. 1999; Card 1999; Harmon et al. 2003; Heckman et al. 2006).[27] This suggests that teacher strikes may also affect the students' labor market outcomes. Table 6 examines this question in detail, showing estimates for the proportion of people in the birth year – birth province – EPH year cell who are unemployed, not in the labor force, and whose main activity is home production.

Looking across the panels, there is clear evidence that exposure to teacher strikes leads to an increase in the proportion that is unemployed. In terms of magnitude, the point estimate in Panel A shows that exposure to ten days of teacher strikes leads to an increase in the proportion of unemployed individuals by 0.7 of a percentage point. This effect is significant at the 1 percent level and represents an effect of approximately 1.39 percent relative to the mean. Comparing Panel B and Panel C reveals that this effect is only present among males.

Teacher strikes also increases the proportion of people whose main activity is home production.[29] The point estimate in Table 6 shows that ten days of teacher strikes increases the proportion of individuals dedicated to home production by 0.18 percent, or 0.9 percent relative to the sample mean. Comparing Panels B and C reveals that this effect is three times larger for women compared to men: Ten days of teacher strikes induces 27 out of every 1,000 females – but only 9 out of every 1,000 males - to move from either working or studying to home production.

With respect to labor force participation, our results in Table 6 suggest that there is no statistically significant effect of teacher strikes on the extensive margin of employment. However, once we control for province-specific linear birth year trends in Section 6.3, we do find significant adverse effects of teacher strike on labor force participation among women. Our inability to detect this effect in our baseline table – we argue – is likely due to strong secular shifts in labor market opportunities that occurred for women over the cohorts we consider (Blau and Kahn 2013; Bick and Bruggeman 2014; Gasparini and Marchioni 2015). The effects that we identify in Section 6.3 suggests that exposure to 10 days of strikes reduces female labor force participation by 0.14 percent relative to the mean shown in Table 4.

[27] However, it is not necessarily the case that adverse educational effects carry over to the labor market (e.g. Böhlmark and Willén 2017).
[29] In our sample, 6 percent are still enrolled in an educational institution and 83 percent of those are enrolled at a university.

iii. Earnings & wages

The adverse employment and education effects identified in Tables 5 and 6 suggest that teacher strikes may have a negative effect on labor market earnings as well. This is examined in Table 7 with respect to log earnings, log wages and the level of earnings.[30] Looking across the columns in Table 7, the results show statistically and economically significant adverse effects of strike exposure on all three income measures for the full sample (Panel A): 10 days of teacher strikes during primary school lead to a reduction in earnings by 0.22 percent (log-specification), in wages by 0.25 percent, and in earnings by USD 1.85 (level-specification).[31] Scaling the point estimates to account for the average level of exposure to teacher strikes during our analysis period (multiplying the point estimates with 8.8) suggests that the average cohort in our sample suffered adverse effects of 1.94, 2.22 and 2.99 percent relative to the sample means, respectively.

Another way to interpret our income estimates is to aggregate them up to the country level and consider the total effect on the Argentinian economy. While such back-of-the-envelope calculations must be cautiously interpreted due to the many factors that cannot be taken into account when performing this exercise, it is informative for understanding the potential magnitude of the effect. With respect to the point estimates in Table 7, this effect is substantial: there are 3,645,970 individuals between the ages of 30 and 40 on the Argentinian labor market, and with an average loss of 88 school days due to teacher strikes, the aggregate earnings loss induced by teacher strikes amounts to USD 712 million. This is equivalent to the cost of raising the average annual employment income of all primary school teachers in Argentina by 19 percent.[32] In terms of policy implications, this suggests that it may be worth raising teacher wages if this will prevent them from going on strike.

A comparison of the gender-specific point estimates in Panels B (males) and C (females) shows that the log earnings and log wage effects are driven entirely by males: 10 days of teacher strikes during primary school leads to a reduction in earnings by 0.21 percent and in wages by 0.32 percent, significant at the 1 percent level. The effect on earnings measured in levels, however, is statistically and economically significant for both genders. The difference between log earnings and earnings is that individuals with zero earnings are excluded from the log-specification. The results in Table 7 therefore suggests that teacher

[30] We include the level of earnings (expressed in 2005 PPP dollars) in addition to the log of earnings as individuals with zero earnings automatically are eliminated from the log specification.
[31] The identified effect on the level of employment income is equivalent to 0.34 percent relative to the mean.
[32] Teachers labor earnings are approximately USD13.000 a year, and there were 289,812 primary school teachers in 2014.

strike exposure increases the proportion of people with zero earnings among both men and women, but conditional on positive earnings, it only impacts males. The female point estimates suggest that, conditional on being employed, wages did not fall, but the decline in the likelihood of receiving positive earnings shifted the wage distribution to the left.

The point estimates in Table 7 can be used to back out the implied rate of return to education in Argentina. The coefficient on annual labor earnings suggests that the return to an additional year of primary education is 6.1 percent.[33] This number is in the lower tail of pre-existing estimates of the private rate of return to education in Argentina: 7-12.5 percent (Kugler and Psacharopoulos 1989; Pessino 1993; Pessino 1996; Gasparini et al. 2001; Galiani and Sanguinetti 2003; Patrinos et al. 2005). Four reasons help explain why the implied rate of return that we obtain in this paper is lower than the pre-existing estimates of the private rate of return to education in Argentina. First, the return to education consists of two components – a human capital component and a signaling component (Lange and Topel 2006). Teacher strikes may negatively affect human capital accumulation due to a reduction in the number of effective instructional days. However, teacher strikes may not affect the signaling value of education as much as the loss of a formal school year would, since it is unlikely that employers remember the level of strikes when the employee was enrolled in primary school.

Second, our estimates represent the intent-to-treat effect of exposure to teacher strikes based on the province that the individuals were born in. As shown in Table 3, not all individuals attend primary school in their birth province. Although the fraction of individuals that attend school in another province is very small, some individuals' treatment status will be misclassified, causing a slight attenuation bias. Third, we have treated all province-specific teacher strikes as affecting all schools in the province, but there is not 100 percent teacher compliance with respect to industrial action. This will again lead to a slight attenuation bias.

Finally, a fraction of individuals in each birth province – birth year – calendar year cell has attended private primary school (approximately 18 percent), and it is unusual for private school teachers to participate in teacher strikes; while public teachers make up approximately 35 percent of all strikes in Argentina, private teachers account for less than 4 percent of all strikes (Chiappe 2011; Etchemendy 2013). As we assign treatment status based on public school teacher strikes, this will again lead to a slight attenuation bias of our point estimates, since individuals that attended private school were not exposed to all of these strikes.

[33] This number is obtained by multiplying the estimated effect of 0.34 percent by 18, as the school year consists of 180 instructional days.

The above factors explain why the implied rate of return to education that we obtain in this paper does not perfectly mirror the pre-existing estimates of the private rate of return to education in Argentina, and why we should not expect this to be the case. This discussion also serves to explain that our point estimates should be viewed as a lower-bound of the effect of teacher strikes and that the likely effect of exposure to teacher strikes is larger.

The results in Table 7 may conceal important heterogeneous treatment effects across the earnings and wage distributions. We explore this possibility in Table 8 for the full sample (Panel A) as well as for males (Panel B) and females (Panel C) separately. Table 8 demonstrates that teacher strikes affect all but the lowest three deciles of the wage and earnings distributions, and that the magnitude of the effect is relatively constant across the different deciles. For males, this indicates that the people in the left tail of the wage distribution would have done equally poorly without teacher strikes, while the rest of the individuals would have done better. For females, we find that the decline in the likelihood of receiving positive earnings moved the wage distribution leftwards, producing a significant effect only for the deciles after which woman participate in the labor force.

iv. Occupational quality, informal employment & hours worked

In addition to the extensive margin employment effects that we identify above, the adverse effect of teacher strikes on earnings could be driven by a reduction in work hours and by affected individuals sorting into lower-quality occupations. This is examined in Table 9, where we look at the effect of teacher strikes on occupational sorting, hours worked and the proportion of people that work in the informal sector. To study occupational sorting, we follow Lovenheim and Willén (2016) and calculate the proportion of workers in one's occupation with more than a high school degree to construct an index of occupation quality. A reduction in this index is interpreted as an occupational downgrading since it implies that one is working with lower-quality colleagues (as measured by their educational attainment). Total hours are defined only for employed workers. Finally, we define a person as holding an informal job if s/he is a salaried employee in a small firm (less than 5 employees), works as self-employed without a university degree, or is a family worker with zero earnings.

The results suggest that being exposed to 10 days of strikes during primary school has no effect on hours worked, but does have a negative effect on occupational sorting.[34] Comparing the gender-specific point estimates in Panel B (males) and Panel C (females) demonstrates that this effect is driven entirely by men. With respect to the average male who was exposed to 88 days of teacher strikes during primary school, the occupational sorting effect represents an effect of 1.32 percent relative to the sample mean in Table 4. The gender-specific results further show that teacher strikes increase the likelihood of working in the informal sector among females but not males. For the average female in our sample who was exposed to the 88 days of teacher strikes during primary school, the increase in the likelihood of working in the informal sector represents an effect of 4.2 percent relative to the mean.

v. Effect of teacher strikes conditional on education attainment

The effect of teacher strikes on employment and earnings can operate through two different human capital mechanisms. First, it can be driven by the reduction in educational attainment (the extensive margin of education) that we identify in Section 6.1 (Table 5). Second, it can be driven by a reduction in the amount of human capital accumulation that is associated with any given level of education (the intensive margin of education). For example, substantial teacher strikes in a given year may lead teachers to lower the examination requirements for a certain cohort in order to account for lost instructional time, so that the extensive margin of education is unaffected while there are adverse effects on the intensive margin.[35]

To obtain suggestive evidence of the relative importance of these two mechanisms, we run individual-level regressions of the main outcomes conditional on educational attainment. The intuition behind this approach is that such regressions eliminate the extensive margin effect of teacher strikes, and the effect that remains is therefore driven, at least in part, by the intensive margin. Table 10 presents results of the effect of teacher strikes holding educational attainment constant. We find that approximately 50 percent of the effect on occupational sorting and earnings among men is explained by the extensive margin, while the other 50 percent is due to intensive margin effects. However, most of the effects on unemployment and home production are explained by the intensive margin. Although the relative importance of the intensive and extensive margin effects appears to differ across the outcomes that we look

[34] The results are robust to alternative measures of occupational quality, such as average wage or years of education in one's occupation.

[35] The results in this section should be interpreted with caution and considered only as suggestive since there is likely selection on unobservables into each of the educational levels as response to teacher strike exposure.

at, the main take-away is that the effect of strike exposure on later-in-life labor market outcomes operates through both intensive and extensive margin education effects.

vi. Socioeconomic & intergenerational effects of teacher strikes

There exists a large literature documenting a strong positive relationship between an individual's education- and labor market outcomes and his socioeconomic position (e.g. Finer and Zolna 2014). Given the adverse education and labor market effects that we identify above, teacher strikes may also impact outcomes such as the likelihood of being married, the probability of being the head of the household (or the spouse to the head of the household), the number of children (conditional on being head or spouse), the age of the oldest child, the educational attainment of the partner, and the household per capita income. [36] Table 11 explores these questions in detail, showing the results from estimation of equation (1) for each of the outcomes mentioned above.

Across the columns in Table 11, there is clear evidence of a negative effect of teacher strikes on the probability of being the household head (or the spouse to the head of the household), and of a positive effect of having children, among females. Relative to the sample means displayed in Table 4, exposure to ten days of teacher strikes leads to a 0.19 percent reduction in the likelihood of being household head and a 0.32 percent increase in the probability having children. That we find effects among females but not among males could be due to the heterogeneous treatment effects identified in Section 6.1: while teacher strike exposure causes males to sort into lower skill occupations, it leads females to move toward home production, potentially lowering their bargaining position in the household (thus leading to a reduction in the probability of being household head) and increasing the time that they can allocate towards non-work tasks (such as raising children).[37]

The results in Table 11 further show that teacher strike exposure affects the marriage market by influencing the characteristics of exposed individuals' partners. Specifically, the results show that the partners of females that were exposed to more days of teacher strikes are less educated: an additional 10 days of strikes leads to a decline in the years of education of

[36] Given the structure of the EPH, we can only identify children of the head, or the spouse of the head, of the household.

[37] It is important to note that the positive effect on the probability of having children does not imply that exposure to teacher strikes induces an increase in total fertility; it could be that affected individuals have the same number of children but that they have them sooner. In an attempt to disentangle this effect, we examine the effect of strike exposure on the age of the oldest child in the household. We find a statistically significant effect of strike exposure on the age of the oldest child in the household, supporting the claim that more exposed cohorts have their first child sooner than less exposed cohorts.

females' partners by 0.037, or 4.7 percent relative to the sample mean when scaled to the average strike exposure of 88 days. We do not find a significant effect among males.

Finally, the point estimates in Table 11 also show that strikes affect per capita family income: the average individual in our sample is exposed to 88 days of teacher strikes, and this is associated with a decline in household per capita income by around 4 percent relative to the sample mean. The effect is not statistically significantly different across genders. For females, this effect seems to be driven by an increase in family household size and by a decline in the earnings of their partners (that are on average less educated). For males, the decline in per capita income is driven by the negative effects of strikes on their individual earnings.

Given that teacher strikes not only has adverse effects on long-run educational attainment and labor market outcomes, but also influences the family planning decisions of females, it follows that there may be important intergenerational treatment effects associated with strikes. Even though data limitations prevent us from exploring such effects in great detail, we can look at two educational outcomes of children to individuals that were exposed to teacher strikes during primary school. First, the probability of not being delayed at school. This variable takes a value of one if the age of the child minus years of education is greater than 6 (age at which children are expected to start primary education), and zero otherwise. Second, the educational gap defined by years of schooling plus 6 minus age.

The point estimates obtained from estimating our main specification with the probability of not being delayed at school and the educational gap as the dependent variables, are displayed in Table 12. Across the table, there is evidence of adverse intergenerational education effects among females but not among males. This is consistent with the heterogeneous treatment effects identified in Section 6.1. In terms of magnitude, being exposed to ten days of teacher strikes during primary school leads to a 0.43 percent increase in the probability that the child is delayed at school relative to the mean (and to an increase in the education gap of 1.45 percent relative to the mean).

Taken together, the above discussion demonstrates that exposure to teacher strikes not only impacts long-term educational attainment and labor market outcomes, but also family planning decisions and the educational outcomes of the affected individuals' children. These results have never been documented before. Due to the scarce literature on this topic, additional research that examines these questions should be encouraged.

6.2 Heterogeneous Treatment Effects

A large literature has documented that human capital accumulates over time, such that human capital obtained at one point in time facilitates further skill attainment later in life (e.g. Heckman et al. 2006). Therefore, early childhood investments are often argued to yield higher returns than education investments that target older children.[38] With respect to the current analysis, this suggests that exposure to teacher strikes in early grades may have larger adverse effects on long-run educational and labor market outcomes.

Table 13 shows the effect of exposure to teacher strikes on the long-term education and labor market outcomes of students based on whether they were exposed to strikes in grades 1 through 4 or in grades 5 through 7. Across the columns in Table 13, there is suggestive evidence that teacher strikes in early grades have noticeably larger adverse effects than strikes in later grades. However, these differences are generally not statistically significant. For example, exposure to ten days of strikes while in grades 1 through 4 leads to a reduction in wage by 0.37 percent. Exposure to teacher strikes while in grades 5 through 7 causes a decline in wage by only 0.17 percent. However, we are unable to reject the null that the difference between the two estimates is zero. Only for two outcomes do we find that the effect of teacher strikes in early school grades is statistically significantly different from the effect of teacher strikes in later school grades: years of education and total earnings for females.

6.3 Robustness & Sensitivity Analysis

The results obtained from our preferred specification support the idea that teacher strikes have adverse effects on long-term educational attainment and labor market outcomes. In this section, we explore evidence on whether these results are driven by other policies, trends or events that are not accounted for by the controls in equation (1).

In Panel A and Panel B of Table 14 we exclude the city of Buenos Aires and the province and city of Buenos Aires, respectively. These geographic areas differ slightly from the rest of Argentina with respect to their institutions and legislation, and account for half the population of the country. The purpose of this exercise is to ensure that our results are not driven exclusively by these geographic areas. Comparing the results in Panel A and Panel B with our baseline results in Section 6.1, it is clear that there are no statistically or

[38] This argument is also based on research that finds young children to be more receptive to learning. See for example Shonkoff and Phillips (2000).

economically significant differences between the point estimates obtained from estimating equation (1) when these regions are included and the point estimates obtained from estimating equation (1) when these regions have been omitted.[39]

In Panel C of Table 14 we re-estimate our preferred model specification without the five provinces that have the highest cross-province mobility rates (Chaco, Corrientes, Misiones, Rio Negro and Santa Cruz). The point estimates produced for this subsample of provinces are not statistically significantly different from our baseline results. This demonstrates that our results are robust to accounting for cross-province mobility.

Panel D of Table 14 eliminates pre-2010 EPH survey years to ensure that our results are robust to a balanced panel of age observations. Despite a dramatic loss of observations (recall that our baseline analysis relies on the 2003-2015 EPH waves), the point estimates are not statistically significantly different when imposing this restriction. This illustrates that our results are robust to having a balanced panel with respect to age observations.

Panel E of Table 14 displays results from estimation of equation (1) when we have reassigned the treatment variable for birth cohort *c* to birth cohort *c-7*. These cohorts are very close in age and are likely exposed to similar province-specific macroeconomic environments. However, the *c-7* cohorts have already completed primary school when the documented teacher strikes took place, and if our baseline estimates successfully isolate the effect of teacher strikes on student outcomes, we should not find any statistically and economically significant effects among these cohorts. Looking across the columns, none of the point estimates are statistically significant. These results are therefore consistent with the identification assumption that the timing of teacher strikes is uncorrelated with prior trends in outcomes across birth cohorts within each province.

Panel F shows results for our preferred specification when province-specific linear birth year trends have been included. These results help us to further examine if our empirical research design has successfully managed to isolate the effect of teacher strikes on student outcomes, or if the coefficient estimates simply are driven by trends in outcomes across birth cohorts within each province. The results from this exercise are not statistically significantly different from our baseline estimates. This demonstrates that our baseline results are not driven by trends in outcomes across birth cohorts within each province.

[39] Even though the effect of exposure to teacher strikes on total earnings among males is smaller in Panel B compared to our baseline estimate, this difference is not statistically significant.

Panel G displays results from estimation of equation (1) when we drop the birth cohort – birth province – calendar year cells that were in the top percent of the teacher strike exposure distribution. Looking across the columns, Panel G shows that the exclusion of outliers does not change our results. There is one exception - the coefficient on total earnings for males is no longer significant. However, this point estimate is imprecisely estimated in our baseline table and was only significant at 10 percent. The coefficient estimates on hourly wages, years of education and occupational sorting are not statistically significantly different when we omit outliers, and they are still significant at the 1 percent level. These results demonstrate that our results are not being driven by outliers.

One of the main threats to valid inference in our paper, despite the inclusion of fixed effects and demographic controls, is that our results are simply picking up differences in outcomes caused by province-specific variation in macroeconomic performance across time. To explore this question, we use post-2003 EPH data (data on local labor markets do not exist before 2003) to explore the relationship between teacher strikes and local labor market conditions. Provided that the relationship between teacher strikes and local labor markets after 2003 is informative of that same relationship during the period 1977-1998, this auxiliary analysis can be used to examine if our results are simply picking up differences in outcomes caused by province-specific variation in macroeconomic performance over time.

The result from this exercise is shown in Table 15. In Column (1) we show the correlation between teacher strikes and the unemployment rate, the average hourly wages and the average per capita family income. In Column (2) we add days of strikes in public administration as well as calendar year and province fixed effects.[40] Our main finding is that, once we control for public administration strikes, province-specific GDP and province and year fixed effects, there is no significant relation between the local labor market climate and teacher strikes. In Table 16 we further show that the inclusion of public administration strikes and province-specific GDP controls have no impact on our main results. Taken together, these results suggest that our identified results are not simply driven by province-specific variation in macroeconomic performance across time.

[40] The results are robust to the inclusion of the 30th and the 70th percentiles of the per capita family income (intended to capture any effect of a change in the distribution of per capita family income). Results are available upon request.

6.4 Short-run effects

In this section we analyze the effect of exposure to teacher strikes on outcomes of students who have just finished primary school.[41] The purpose of this exercise is to examine if the adverse effects of strike exposure that we have identified above are present immediately after the children have been exposed to strikes, or if the effects develop over time. We use the 2003-2015 EPH waves for children between 12 and 17 years old to perform this analysis.[42] We concentrate on educational outcomes since most of these individuals have not yet entered the labor market. These outcomes are: the likelihood of having attended primary school, the probability of attending a public institution, years of education, the likelihood that the main activity is home production, and the likelihood of being enrolled in school. We perform this analysis on the individual level to control for household characteristics.[43]

Table 17 displays the results for each one of the outcome variables using two different specifications. Column (1) incorporates the same controls as in our preferred specification.[44] Column (2) incorporates additional local labor market controls (the unemployment rate and the average wage in each province-year) and family characteristics (4 dummies for province-specific quartiles of per capita family income and 5 dummies for the maximum educational level of the head, or spouse to the head, of the household: primary education or less, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary).

With respect to females, the results in Table 17 shows that there is a decline in public education enrollment of 0.59 of a percentage point, or 0.74 percent relative to the sample mean. This effect increases to 4.2 percent relative to the mean when we scale the coefficient to account for the average level of strike exposure among these individuals (57 days). For males, the effect of exposure to 10 days of strikes during primary education reduces the years of education by 0.029 (0.37 percent relative to the mean), increases the likelihood of home production by 0.0021 (3.45 percent relative to the mean) and decreases the probability of being enrolled by 0.0040 (5.03 percent relative to the mean). These results indicate that the negative effects of teacher strikes during primary school on educational attainment are already visible at the secondary level, in particular for men.

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[41] Due to educational reforms during the past two decades, grade 7 became a part of secondary education in 2002, and mandatory education was extended from 7 to 12 years in 1998. In this section the treatment variable is still defined as the days of strike while students were in primary school, which is now when the children were between 6 and 11 years old.

[42] We exclude cohorts from 1986 to 1990 since the educational reform was taking place at a different rate in each province Gasparini et al. (2015).

[43] These results are robust to estimation at the aggregate level used in our main analysis. These results are available upon request.

[44] Except for GDP at the province level for which there is not reliable data available in recent years.

In Section 2.3 we note that there may be heterogeneous treatment effects of teacher strike exposure with respect to the socioeconomic characteristics of the student's parents: wealthy parents can afford to move their children to private institutions if they believe the strikes hurt their children, and more educated parents are more likely to be capable to replace lost instructional days with home schooling. Even though we do not have information on parental wealth and educational attainment for the individuals included in our main analysis, we can examine this for children that are between 12-17 years old. In Table 18, we estimate the effect of teacher strike exposure by per capita family income and maximum years of education of the head, or the spouse to the head, of the household. The equation that has been used to obtain the results shown in Panel A includes dummies of maximum education of head, or spouse to the head, of the household, as well as interactions between the treatment variable and these dummies. The model underlying the results presented in Panel B includes indicator variables for province-specific quartiles of per capita family income as well as interactions between the treatment variable and these dummies. Consistent with our predictions, we find clear evidence that the most affected students are those from the most socioeconomically disadvantaged households.

7. Discussion and Conclusion

Teacher industrial action is a prevalent feature of public education systems across the globe. Despite a large theoretical literature on labor strikes and a reignited debate over the role of teachers' unions in education, there is a lack of empirical research that credibly evaluates the effect of teacher strikes on student outcomes. This paper contributes to the literature by providing a detailed analysis of the effect of exposure to teacher strikes during primary school on long-run educational attainment and labor market outcomes.

Our analysis reveals that there are adverse effects of exposure to teacher strikes on long-run educational attainment and labor market outcomes for both males and females. For males, we find that exposure to teacher strikes during primary school leads to a reduction in educational attainment, an increase in the likelihood of being unemployed, occupational downgrading, and has adverse effects on both labor market earnings and hourly wages. For females, teacher strikes reduce educational attainment in a way similar to that of men. We find a reduction in the level of earnings among females as well, but teacher strikes do not affect the wages of females who are employed. We show that this is because teacher strikes induce females to sort into home production. Our analysis reveals that teacher strikes affect women on several additional socioeconomic dimensions as well. Specifically, females exposed to teacher strikes during primary school have more children, less educated partners, and lower per capita family income. We argue that some of these effects are driven by a decline in female's bargain power within the household. By looking at 12-17 years old, we demonstrate that the negative educational effects of teacher strikes are already visible at the secondary level, and that these effects are concentrated among children from the most vulnerable households.

The prevalence of teacher strikes in Argentina means the effect of teacher strikes on the economy as a whole is substantial: A back-of-the-envelope calculation suggests an aggregate annual earnings loss of $712 million. This is equivalent to the cost of raising the average employment income of all primary school teachers in Argentina by 19 percent. In terms of policy implications, this suggests that it may be worth raising teacher wages if this will prevent them from going on strike.

Taken together, our results stress the importance of stable labor relations between government and industry and emphasize the necessity of creating a stable bargaining environment that reduces the number of days of teacher strikes that students are exposed to. Given that the negative effects that we identify last for years and even generations, both unions and government should make substantial attempts to limit the prevalence of teacher strikes. One policy could be to introduce labor contracts that extend over several years, and only allow teachers to strike if a bargaining impasse is reached when renewing these multi-year contracts. This would eliminate sporadic teacher strikes while still allowing teachers to use industrial action as a tool to ensure fair contracts.

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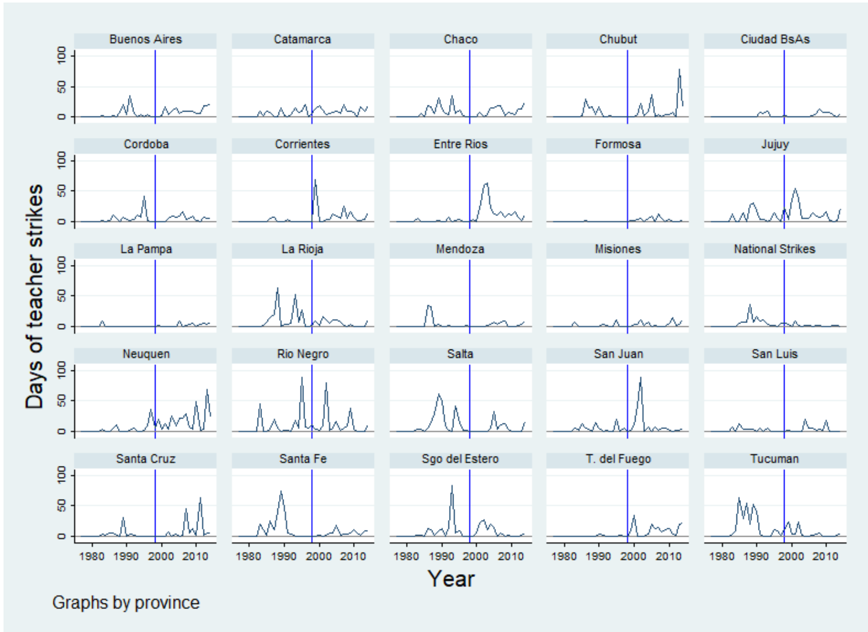
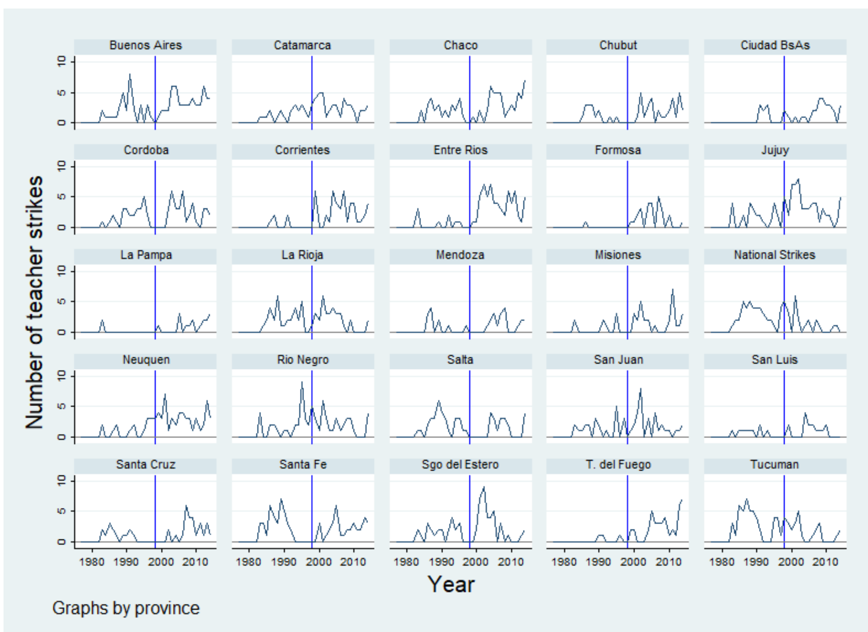
Figure 1: Variation in Teacher Strikes 1977-2014



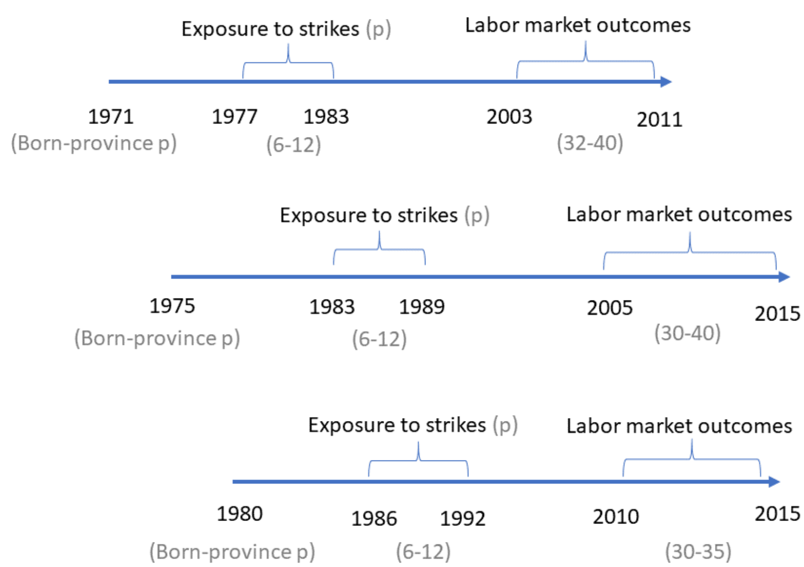
Panel A: Days of teacher strikes



Panel B: Number of teacher strikes

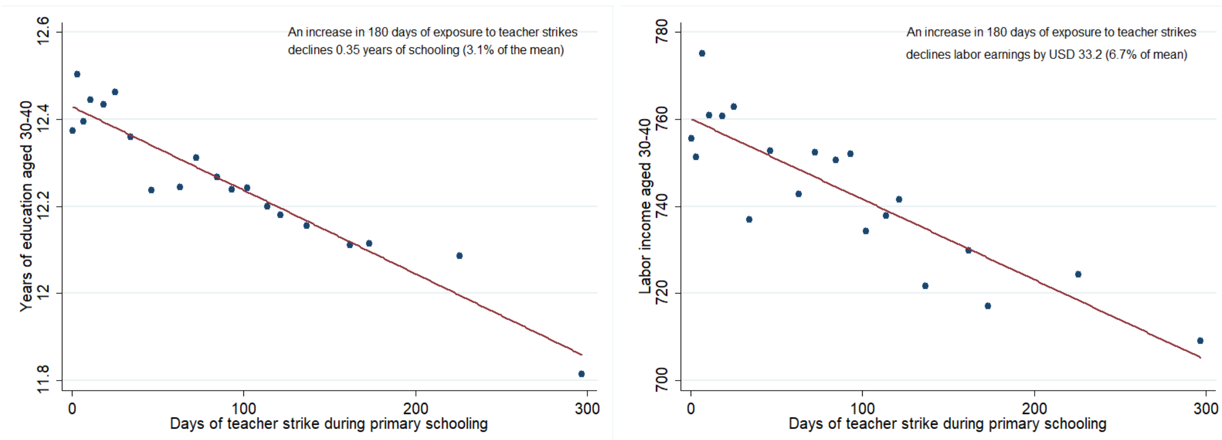
Notes: Authors' tabulations from annual reports on the Argentine economy published by Consejo Técnico de Inversiones (1977-2014). Panel A shows the evolution of days teacher strikes for each province and at a national level. Panel B displays the number of teacher strikes. The vertical line indicates the two sub-samples used for the estimation of long-run (left) and short-run (right) outcomes.

Figure 2: Data structure for a subsample of birth cohorts



Notes: Example of three cohorts that are part of our main analysis.

Figure 3: Suggestive evidence: correlation between teacher strikes and student Outcomes



A. Years of Education B. Earnings

Notes: The figure is a binned scatter plot. The horizontal axis shows the days of teacher strikes during primary education, which varies at birth year- birth province level. The vertical axis of Panel A contains the average years of education and Panel B the average labor income for each birth year- birth province-survey year cell, after controlling for province, cohort and survey year fixed effects. Data is grouped on 20 intervals of equal number of observations according to days of exposure to teacher strikes. Each point correspond to the group average of the variable in the vertical axes. 180 days of teacher strikes is equivalent to a full year of primary school and the difference between the 10th and the 90th percentile of teacher strike exposure among the individuals included in our sample.

Table 1: Days of teacher strikes during primary school by birth cohort and birth province

	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985	**Mean**
Buenos Aires	2	2	3	5	6	14	35	36	71	76	74	77	69	52	50	**38**
Catamarca	9	11	21	29	29	30	45	38	36	29	35	42	51	56	55	**34**
Chaco	0	5	5	23	40	45	76	88	88	91	108	97	103	74	62	**60**
Chubut	0	0	2	31	45	62	65	82	82	80	53	39	23	20	3	**39**
Ciudad Bs.As.	0	0	0	0	0	0	0	0	7	13	22	22	22	22	22	**9**
Cordoba	1	1	2	13	19	19	27	30	32	34	34	35	76	70	66	**31**
Corrientes	0	0	0	5	12	12	12	12	16	16	11	4	4	4	4	**7**
Entre Rios	6	6	6	6	6	6	8	2	2	10	10	11	15	13	13	**8**
Formosa	0	0	0	2	2	2	2	2	2	2	0	0	0	0	0	**1**
Jujuy	12	12	12	27	27	54	85	91	95	98	83	87	75	49	31	**56**
La Pampa	9	9	9	9	9	9	9	0	0	0	0	0	0	0	0	**4**
La Rioja	0	1	9	24	44	107	107	110	112	110	147	134	99	98	95	**80**
Mendoza	0	0	0	35	68	68	72	72	72	74	39	6	6	2	3	**34**
Misiones	7	7	7	7	7	7	7	0	3	5	5	5	15	15	15	**7**
Neuquen	4	4	4	9	19	19	19	15	17	22	17	7	9	17	53	**16**
Rio Negro	45	45	45	49	68	73	73	30	31	31	45	31	114	121	125	**62**
Salta	4	8	8	13	27	56	118	163	168	170	165	193	178	117	69	**97**
San Juan	5	7	19	23	27	27	41	41	40	30	25	21	40	26	27	**27**
San Luis	7	7	19	22	25	28	31	24	29	17	19	16	13	10	10	**18**
Santa Cruz	4	6	12	17	19	19	49	46	47	42	37	35	35	5	4	**25**
Santa Fe	19	29	31	56	67	106	180	207	203	205	180	169	130	56	10	**110**
Sgo del Estero	2	3	3	16	27	29	38	48	47	62	132	126	132	123	111	**60**
T. Del Fuego	0	0	0	0	0	0	0	4	6	6	6	6	6	8	4	**3**
Tucuman	4	13	76	105	159	179	232	269	264	201	172	118	109	65	26	**133**
Mean	**6**	**7**	**12**	**22**	**31**	**40**	**55**	**59**	**61**	**59**	**59**	**53**	**55**	**43**	**36**	40

Notes: Authors' tabulations from annual reports on the Argentine economy published by Consejo Técnico de Inversiones (1977-1998). The table shows the total days of exposure to teacher strikes at ages 6-12 for each birth year- birth province cell. Cohorts 71-85 correspond to the 30-40 year old respondents in the 2003-2015 EPH for which outcomes variables are available.

Table 2: Birth Cohorts by Age in our sample by EPH Year

Age	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
30	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985
31	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984
32	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983
33	-	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982
34	-	-	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981
35	-	-	-	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980
36	-	-	-	-	1971	1972	1973	1974	1975	1976	1977	1978	1979
37	-	-	-	-	-	1971	1972	1973	1974	1975	1976	1977	1978
38	-	-	-	-	-	-	1971	1972	1973	1974	1975	1976	1977
39	-	-	-	-	-	-	-	1971	1972	1973	1974	1975	1976
40	-	-	-	-	-	-	-	-	1971	1972	1973	1974	1975

Notes: Authors' tabulations from 2003-2015 EPH data on 30-40 year old respondents.

Table 3: Cross-province mobility of 13 year olds

Province	Fraction Non-movers
Buenos Aires	0.979
Catamarca	0.963
Chaco	**0.855**
Chubut	0.930
Ciudad Bs.As.	0.999
Cordoba	0.947
Corrientes	**0.850**
Entre Rios	0.905
Formosa	0.942
Jujuy	0.932
La Pampa	0.952
La Rioja	0.968
Mendoza	0.947
Misiones	**0.836**
Neuquen	0.979
Rio Negro	**0.715**
Salta	0.943
San Juan	0.949
San Luis	0.945
Santa Cruz	**0.835**
Santa Fe	0.975
Sgo del Estero	0.942
T. del Fuego	0.943
Tucuman	0.952

Notes: Authors' tabulations from 2003-2015 EPH data on 13 year old respondents. The table shows the fraction of 13 year olds during 2003-2015 that live in the same province they were born. Bold numbers represents provinces with fraction of non-movers higher than 0.9.

Table 4: Dependant variable means

	All	**Male**	**Female**
Panel A: Educational Attainment			
Secondary Education Completed	0.589	0.559	0.620
Years of Education	11.455	11.178	11.731
Tertiery Education Completed	0.207	0.166	0.248
Panel B: Employment			
Unemployment	0.054	0.042	0.066
Not in Labor Force	0.177	0.041	0.312
Home Production	0.199	0.069	0.329
Informal Sector	0.332	0.309	0.354
Hours Worked	31.746	42.265	21.239
Occupational Sorting	0.230	0.177	0.284
Panel C: Wage and Earnings			
Log Total Earnings	6.306	6.489	6.123
Total Earnings	550.4	731.8	372.3
Log Wage	1.256	1.255	1.257
Panel D: Other Socioeconomic Outcomes			
Head of Household or Spouse	0.772	0.743	0.801
Married	0.702	0.716	0.688
Number of Children	1.512	1.353	1.671
Log Per Capita Family Income	6.720	6.791	6.650
Years of Schooling of Partner	11.044	11.732	10.357
Age of older kid	11.824	11.331	12.315
Panel D: Intergenerational Outcomes			
Not Delayed at School	0.721	0.728	0.714
Gap in Years of Education	-0.482	-0.462	-0.503

Notes: Authors' tabulations from 2003-2015 EPH data on 30-40 years old respondents from 1971-1985 cohorts. Home production is defined as neither working nor studying. Informality is defined as the share of employed workers that are salaried employee in a small firm (less than 5 employees), or works as self-employed without a university degree, or is a family worker with zero earnings. Occupational sorting is evaluated by constructing an index of occupation quality based on the proportion of workers in each occupation with more than a high school degree. Not being delayed at school is defined as a dummy variable takes the value of one if the age of the child minus years of education plus 6 is greater than zero, and it takes the value of zero otherwise. The educational gap defined by years of schooling plus 6 minus age.

Table 5: Effect of Strike Exposure on Educational Attainment

	High School Diploma			College Degree			Years of Schooling		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Panel A: All									
Strike Exposure	-0.0030***	-0.0028***	-0.0029***	-0.0014***	-0.0015***	-0.0017***	-0.0233***	-0.0240***	-0.0254***
	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0006)	(0.0005)	(0.0046)	(0.0048)	(0.0045)
% Effect	-0.51%	-0.48%	-0.49%	-0.68%	-0.73%	-0.82%	-0.20%	-0.21%	-0.22%
Panel B: Males									
Strike Exposure	-0.0032***	-0.0030***	-0.0029***	-0.0021***	-0.0024***	-0.0023***	-0.0255***	-0.0262***	-0.0262***
	(0.0009)	(0.0008)	(0.0009)	(0.0006)	(0.0006)	(0.0007)	(0.0060)	(0.0063)	(0.0068)
% Effect	-0.57%	-0.54%	-0.52%	-1.27%	-1.45%	-1.39%	-0.23%	-0.23%	-0.23%
Panel C: Females									
Strike Exposure	-0.0027***	-0.0026***	-0.0029***	-0.0007	-0.0008	-0.0010	-0.0209***	-0.0217***	-0.0238***
	(0.0008)	(0.0008)	(0.0008)	(0.0007)	(0.0008)	(0.0008)	(0.0063)	(0.0064)	(0.0061)
% Effect	-0.44%	-0.42%	-0.47%	-0.28%	-0.32%	-0.40%	-0.18%	-0.18%	-0.20%
Cohort FE	X	X	X	X	X	X	X	X	X
Calender Year FE	X	X	X	X	X	X	X	X	X
Province FE	X	X	X	X	X	X	X	X	X
Province-Specific Time Trends		X			X			X	
Cohort-Specific Time Trends		X			X			X	
Year-by-Cohort FE			X			X			X
Year-by-Province FE			X			X			X

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents. Column (1) controls for birth province, birth year and EPH survey year fixed effects as well as local GDP and exposure to public administration strikes. Cohort-specific and a province-specific linear time trends are added in Column (2). In Column (3) the linear time trends are replaced for birth province-by-EPH survey year and birth year-by-EPH survey year fixed effects. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province- EPH year cell. The coefficient is interpret as the effect of being exposed to teacher strikes for ten additional days during primary school. Standard errors are clustered at the birth province-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 6: Effect of Strike Exposure on Employment

	Unemployed			Not in Labor Force			Home Production		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Panel A: All									
Strike Exposure	0.0005	0.0008**	0.0007***	0.0003	0.0002	0.0003	0.0015***	0.0018***	0.0018***
	(0.0004)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0004)	(0.0004)
% Effect	0.93%	1.49%	1.30%	0.17%	0.11%	0.17%	0.75%	0.90%	0.90%
Panel B: Males									
Strike Exposure	0.0005*	0.0008**	0.0007**	-0.0003	-0.0004	-0.0003	0.0006	0.0009**	0.0009**
	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0004)
% Effect	1.19%	1.91%	1.67%	-0.73%	-0.97%	-0.73%	0.87%	1.30%	1.30%
Panel C: Females									
Strike Exposure	0.0004	0.0009	0.0008	0.0008	0.0007	0.0009	0.0022***	0.0027***	0.0028***
	(0.0006)	(0.0005)	(0.0005)	(0.0007)	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
% Effect	0.61%	1.37%	1.22%	0.26%	0.22%	0.29%	0.67%	0.82%	0.85%
Cohort FE	X	X	X	X	X	X	X	X	X
Calender Year FE	X	X	X	X	X	X	X	X	X
Province FE	X	X	X	X	X	X	X	X	X
Province-Specific Time Trends		X			X			X	
Cohort-Specific Time Trends		X			X			X	
Year-by-Cohort FE			X			X			X
Year-by-Province FE			X			X			X

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents. Column (1) controls for birth province, birth year and EPH survey year fixed effects as well as local GDP and exposure to public administration strikes. Cohort-specific and a province-specific linear time trends are added in Column (2). In Column (3) the linear time trends are replaced for birth province-by-EPH survey year and birth year-by-EPH survey year fixed effects. Home production is defined as neither working nor studying. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-EPH year cell. The coefficient is interpret as the effect of being exposed to teacher strikes for ten extra days during primary school. Standard errors are clustered at the birth province-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 7: Effect of Strike Exposure on Wages and Earnings

	Log Earnings			Log Wages			Total Earnings		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Panel A: All									
Strike Exposure	-0.0020*	-0.0022**	-0.0021**	-0.0018**	-0.0025***	-0.0024***	-1.5925**	-1.8494***	-1.7583***
	(0.0012)	(0.0011)	(0.0010)	(0.0009)	(0.0008)	(0.0008)	(0.6370)	(0.6347)	(0.5978)
% Effect	-	-	-	-	-	-	-0.29%	-0.34%	-0.32%
Panel B: Males									
Strike Exposure	-0.0017	-0.0021*	-0.0023**	-0.0026**	-0.0032***	-0.0032***	-1.6089	-1.7039*	-1.6533*
	(0.0013)	(0.0011)	(0.0010)	(0.0011)	(0.0010)	(0.0010)	(1.0779)	(0.9884)	(0.9671)
% Effect	-	-	-	-	-	-	-0.22%	-0.23%	-0.23%
Panel C: Females									
Strike Exposure	-0.0023	-0.0022	-0.0019	-0.0011	-0.0019	-0.0017	-1.5627*	-1.9064**	-1.7716**
	(0.0020)	(0.0018)	(0.0016)	(0.0014)	(0.0014)	(0.0013)	(0.8494)	(0.8684)	(0.8440)
% Effect	-	-	-	-	-	-	-0.42%	-0.51%	-0.48%
Cohort FE	X	X	X	X	X	X	X	X	X
Calender Year FE	X	X	X	X	X	X	X	X	X
Province FE	X	X	X	X	X	X	X	X	X
Province-Specific Time Trends		X			X			X	
Cohort-Specific Time Trends		X			X			X	
Year-by-Cohort FE			X			X			X
Year-by-Province FE			X			X			X

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents. Column (1) controls for birth province, birth year and EPH survey year fixed effects as well as local GDP and exposure to public administration strikes. Cohort-specific and a province-specific linear time trends are added in Column (2). In Column (3) the linear time trends are replaced for birth province-by-EPH survey year and birth year-by-EPH survey year fixed effects. The outcomes are: log of individual labor earnings, log of wages, and the level individual labor earnings (including zeros). The % effect is drooped for log variables given that the point estimate is already interpreted as a percentage change. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-EPH year cell. The coefficient is interpret as the effect of being exposed to teacher strikes for ten extra days during primary school. Standard errors are clustered at the birth province-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 8: Heterogeneous Treatment Effects. Earnings and Wages

	Percentile (Total Earnings)								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
Panel A: All									
Strike Exposure	0.1890	0.0324	-0.6847	-1.5897**	-1.9398**	-2.2511**	-3.5008***	-2.8398***	-3.7777*
	(0.5968)	(0.4440)	(0.5291)	(0.6785)	(0.7961)	(0.9048)	(1.0053)	(1.0738)	(1.9680)
% Effect	0.20%	0.02%	-0.27%	-0.46%	-0.43%	-0.40%	-0.50%	-0.33%	-0.33%
Panel B: Males									
Strike Exposure	0.4344	-0.2033	-1.3683	-1.1062	-1.3523	-2.1281**	-3.1197**	-2.3928	-5.1729*
	(1.2660)	(0.8985)	(0.8652)	(0.9193)	(0.9001)	(1.0441)	(1.3267)	(1.6405)	(2.6490)
% Effect	0.25%	-0.06%	-0.31%	-0.20%	-0.21%	-0.28%	-0.35%	-0.22%	-0.38%
Panel C: Females									
Strike Exposure	0.0938	0.2931	-0.0724	-2.0409*	-2.4561*	-2.3283	-3.7698**	-3.1255**	-2.3525
	(0.1028)	(0.2940)	(0.6572)	(1.1244)	(1.4360)	(1.5408)	(1.4862)	(1.4404)	(2.9050)
% Effect	0.83%	0.98%	-0.10%	-1.39%	-0.98%	-0.63%	-0.75%	-0.47%	-0.26%

	Percentile (log wages)								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
Panel A: All									
Strike Exposure	0.0013	-0.0009	-0.0022*	-0.0031***	-0.0034***	-0.0041***	-0.0035***	-0.0034***	-0.0029***
	(0.0016)	(0.0014)	(0.0012)	(0.0011)	(0.0011)	(0.0010)	(0.0011)	(0.0010)	(0.0011)
Panel B: Males									
Strike Exposure	0.0019	-0.0025	-0.0040***	-0.0041***	-0.0043***	-0.0048***	-0.0045***	-0.0037**	-0.0039**
	(0.0019)	(0.0015)	(0.0014)	(0.0013)	(0.0014)	(0.0013)	(0.0014)	(0.0016)	(0.0018)
Panel C: Females									
Strike Exposure	0.0010	0.0006	-0.0004	-0.0021	-0.0025	-0.0035**	-0.0026	-0.0034**	-0.0022
	(0.0024)	(0.0021)	(0.0019)	(0.0018)	(0.0016)	(0.0016)	(0.0017)	(0.0017)	(0.0020)

Notes: Authors' estimation using 2003-2015 EPH data on 30-to 40 year old respondents. Each column represents the results from estimation of equation (1) for different deciles of the total labor earning and log wage distribution at the birth year-birth province- EPH year level. All outcomes are expressed in 2005 PPP dollars. The % effect is drooped for log wage given that the point estimate is already interpreted as a percentage change. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-year. The coefficient is interpret as the effect of being exposed to teacher strikes for ten extra days during primary school. Standard errors are clustered at the birth province-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 9: Effect of Strike Exposure on Occupational Quality and Work Hours

	Occupational Sorting			Total Hours			Informal		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Panel A: All									
Strike Exposure	-0.0008***	-0.0008**	-0.0009***	-0.0259	-0.0198	-0.0164	0.0008	0.0007	0.0010
	(0.0003)	(0.0003)	(0.0003)	(0.0221)	(0.0224)	(0.0223)	(0.0006)	(0.0007)	(0.0006)
% Effect	-0.35%	-0.35%	-0.39%	-0.08%	-0.06%	-0.05%	0.24%	0.21%	0.30%
Panel B: Males									
Strike Exposure	-0.0016***	-0.0015***	-0.0014***	-0.0109	-0.0111	-0.0099	-0.0001	-0.0002	-0.0001
	(0.0003)	(0.0003)	(0.0003)	(0.0345)	(0.0368)	(0.0364)	(0.0007)	(0.0007)	(0.0007)
% Effect	-0.91%	-0.85%	-0.79%	-0.03%	-0.03%	-0.02%	-0.03%	-0.06%	-0.03%
Panel C: Females									
Strike Exposure	-0.0002	-0.0003	-0.0004	-0.0366	-0.0262	-0.0194	0.0016*	0.0017*	0.0019**
	(0.0005)	(0.0006)	(0.0005)	(0.0354)	(0.0375)	(0.0396)	(0.0010)	(0.0010)	(0.0009)
% Effect	-0.07%	-0.11%	-0.14%	-0.17%	-0.12%	-0.09%	0.45%	0.48%	0.54%
Cohort FE	X	X	X	X	X	X	X	X	X
Calender Year FE	X	X	X	X	X	X	X	X	X
Province FE	X	X	X	X	X	X	X	X	X
Province-Specific Time Trends		X			X			X	
Cohort-Specific Time Trends		X			X			X	
Year-by-Cohort FE			X			X			X
Year-by-Province FE			X			X			X

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents. Column (1) controls for birth province, birth year and EPH survey year fixed effects as well as local GDP and exposure to public administration strikes. Cohort-specific and a province-specific linear time trends are added in Column (2). In Column (3) the linear time trends are replaced for birth province-by-EPH survey year and birth year-by-EPH survey year fixed effects. Occupational sorting is evaluated by constructing an index of occupation quality based on the proportion of workers in each occupation with more than a high school degree. Total hours are only defined for employed workers. Informality is defined as the share of employed workers that are salaried employee in a small firm (less than 5 employees), or works as self-employed without a university degree, or is a family worker with zero earnings. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-EPH year cell. The coefficient is interpret as the effect of being exposed to teacher strikes for ten extra days during primary school. Standard errors are clustered at the birth province-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 10: Effect of Strike Exposure Holding Educational Attainment Constant

	Occupational Sorting	Log Wage	Total Earnings	Unemployed	Home Production
Panel A: All					
Strike Exposure	-0.0004*	-0.0014*	-0.8481	0.0008***	0.0017***
	(0.0002)	(0.0008)	(0.5503)	(0.0003)	(0.0004)
% Effect	-0.17%	-	-0.15%	1.49%	0.85%
% Of total effect of strikes	*50.0%*	*58.33%*	*48.2%*	*114.3%*	*94.4%*
Panel B: Males					
Strike Exposure	-0.0008***	-0.0019*	-0.8220	0.0008**	0.0009**
	(0.0003)	(0.0010)	(0.9023)	(0.0003)	(0.0004)
% Effect	-0.45%	-	-0.11%	1.91%	1.30%
% Of total effect of strikes	*50.0%*	*59.38%*	*49.7%*	*114.3%*	*100.0%*
Panel C: Females					
Strike Exposure	0.0002	-0.0007	-0.8949	0.0010*	0.0025***
	(0.0004)	(0.0011)	(0.6313)	(0.0006)	(0.0007)
% Effect	0.07%	-	-0.24%	1.53%	0.76%
% Of total effect of strikes	*-100.0%*	*41.18%*	*50.5%*	*125.0%*	*89.3%*

Notes: Authors' estimation using 2003-2015 EPH data on 30-to 40 year old respondents. Each column depicts the results from estimation of equation (1) using individual-level data in order to control for educational attainment. A full set of dummies for years of education are included as control. The coefficient is interpret as the effect of being exposed to teacher strikes for ten extra days during primary school, holding constant the level of educational attainment. Standard errors are clustered at the birth province-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 11: Effect of Strike Exposure on Other Socioeconomic Outcomes

	Head of Household			Married			Number of Kids		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Panel A: All									
Strike Exposure	-0.0006	-0.0012**	-0.0015***	0.0002	-0.0005	-0.0004	0.0048**	0.0035**	0.0036**
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0005)	(0.0022)	(0.0017)	(0.0015)
% Effect	-0.08%	-0.16%	-0.19%	0.03%	-0.07%	-0.06%	-0.29%	-0.34%	-0.32%
Panel B: Males									
Strike Exposure	-0.0006	-0.0015*	-0.0019**	0.0002	-0.0005	-0.0006	0.0053**	0.0030	0.0027
	(0.0009)	(0.0008)	(0.0008)	(0.0009)	(0.0007)	(0.0007)	(0.0024)	(0.0022)	(0.0023)
% Effect	-0.08%	-0.20%	-0.26%	0.03%	-0.07%	-0.08%	-0.22%	-0.23%	-0.23%
Panel C: Females									
Strike Exposure	-0.0005	-0.0010	-0.0011*	0.0003	-0.0004	-0.0002	0.0044	0.0041	0.0044*
	(0.0007)	(0.0007)	(0.0006)	(0.0007)	(0.0007)	(0.0007)	(0.0030)	(0.0025)	(0.0025)
% Effect	-0.06%	-0.12%	-0.14%	0.04%	-0.06%	-0.03%	-0.42%	-0.51%	-0.48%
Cohort FE	X	X	X	X	X	X	X	X	X
Calender Year FE	X	X	X	X	X	X	X	X	X
Province FE	X	X	X	X	X	X	X	X	X
Province-Specific Time Trends		X			X			X	
Cohort-Specific Time Trends		X			X			X	
Year-by-Cohort FE			X			X			X
Year-by-Province FE			X			X			X

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents. Column (1) controls for birth province, birth year and EPH survey year fixed effects as well as local GDP and exposure to public administration strikes. Cohort-specific and a province-specific linear time trends are added in Column (2). In Column (3) the linear time trends are replaced for birth province-by-EPH survey year and birth year-by-EPH survey year fixed effects. Head of household is defined as head or spouse of the head. Number of children is conditional on being head or spouse. The age of the oldest child is conditional on having children and only defined for head or spouse of the household. The educational attainment of the partner is defined for heads of households or spouses to heads of households. ith spouse of for spouses). Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-EPH year cell. The coefficient is interpret as the effect of being exposed to teacher strikes for ten extra days during primary school. Standard errors are clustered at the birth province-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 11: Effect of Strike Exposure on Other Socioeconomic Outcomes (continue)

	Age of older kid			Per Capita Family Income			Years of Schooling of Partner		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Panel A: All									
Strike Exposure	0.0117*	0.0141**	0.0139**	-0.0044***	-0.0045***	-0.0045***	-0.0210***	-0.0224***	-0.0230***
	(0.0063)	(0.0064)	(0.0062)	(0.0013)	(0.0012)	(0.0010)	(0.0056)	(0.0058)	(0.0057)
% Effect	0.10%	0.12%	0.12%				-0.24%	-0.38%	-0.41%
Panel B: Males									
Strike Exposure	0.0099	0.0083	0.0089	-0.0049***	-0.0046***	-0.0041***	-0.0066	-0.0068	-0.0067
	(0.0088)	(0.0093)	(0.0098)	(0.0015)	(0.0015)	(0.0014)	(0.0073)	(0.0076)	(0.0083)
% Effect	0.08%	0.07%	0.08%				-0.10%	-0.10%	-0.10%
Panel C: Females									
Strike Exposure	0.0129	0.0195**	0.0185**	-0.0039**	-0.0045***	-0.0049***	-0.0343***	-0.0370***	-0.0374***
	(0.0081)	(0.0088)	(0.0089)	(0.0017)	(0.0016)	(0.0016)	(0.0086)	(0.0084)	(0.0086)
% Effect	0.11%	0.16%	0.16%				-0.50%	-0.53%	-0.54%
Cohort FE	X	X	X	X	X	X	X	X	X
Calender Year FE	X	X	X	X	X	X	X	X	X
Province FE	X	X	X	X	X	X	X	X	X
Province-Specific Time Trends	X	X	X		X	X	X	X	X
Cohort-Specific Time Trends		X			X		X		X
Year-by-Cohort Fixed Effects	X		X			X		X	

Table 12: Intergenerational Treatment Effects

	Not Delayed at School			Gap in Years of Education		
	(1)	(2)	(3)	(1)	(2)	(3)
Panel A: All						
Strike Exposure	-0.0014	-0.0011	-0.0009	-0.0045	-0.0028	-0.0022
	(0.0009)	(0.0008)	(0.0007)	(0.0030)	(0.0023)	(0.0021)
% Effect	-0.19%	-0.15%	-0.12%	0.93%	0.58%	0.46%
Panel B: Males						
Strike Exposure	0.0006	0.0012	0.0012	-0.0012	0.0022	0.0025
	(0.0014)	(0.0013)	(0.0013)	(0.0044)	(0.0038)	(0.0038)
% Effect	0.08%	0.16%	0.16%	0.26%	-0.48%	-0.54%
Panel C: Females						
Strike Exposure	-0.0031***	-0.0031***	-0.0029***	-0.0074**	-0.0073***	-0.0062***
	(0.0009)	(0.0008)	(0.0008)	(0.0029)	(0.0024)	(0.0023)
% Effect	-0.43%	-0.43%	-0.41%	1.47%	1.45%	1.23%
Cohort FE	X	X	X	X	X	X
Calender Year FE	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Province-Specific Time Trends		X			X	
Cohort-Specific Time Trends		X			X	
Year-by-Cohort FE			X			X
Year-by-Province FE			X			X

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents. Column (1) controls for birth province, birth year and EPH survey year fixed effects as well as local GDP and exposure to public administration strikes. Cohort-specific and a province-specific linear time trends are added in Column (2). In Column (3) the linear time trends are replaced for birth province-by-EPH survey year and birth year-by-EPH survey year fixed effects. Not being delayed at school is a dummy variable that takes the value of one if the age of the child minus years of education plus 6 is greater than zero. The educational gap is defined by years of schooling plus 6 minus age. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-year. The coefficient is interpret as the effect of being exposed to teacher strikes for ten extra days during primary school. Standard errors are clustered at the birth province-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 13: Heterogeneous Treatment Effects of Strike Exposure by School Grade

	Years of Education	Occupational Sorting	Log Wage	Total Earnings	Unemploy.	Home Production
Panel A: All						
Strike Exposure 1-4 grade	-0.0328***	-0.0013***	-0.0037***	-2.5668***	0.0012***	0.0019***
	(0.0061)	(0.0004)	(0.0012)	(0.8954)	(0.0004)	(0.0006)
Strike Exposure 5-7 grade	-0.0181***	-0.0005	-0.0017*	-1.3668*	0.0006	0.0017***
	(0.0050)	(0.0004)	(0.0010)	(0.7052)	(0.0004)	(0.0005)
Panel B: Males						
Strike Exposure 1-4 grade	-0.0295***	-0.0015***	-0.0039***	-1.5495	0.0011**	0.0010*
	(0.0087)	(0.0004)	(0.0014)	(1.2937)	(0.0005)	(0.0005)
Strike Exposure 5-7 grade	-0.0240***	-0.0014***	-0.0027**	-1.8088	0.0006	0.0008
	(0.0067)	(0.0004)	(0.0013)	(1.1936)	(0.0004)	(0.0005)
Panel C: Females						
Strike Exposure 1-4 grade	-0.0355***	-0.0011	-0.0035*	-3.2691***	0.0013**	0.0027**
	(0.0073)	(0.0007)	(0.0018)	(1.1191)	(0.0006)	(0.0011)
Strike Exposure 5-7 grade	-0.0126*	0.0003	-0.0008	-0.9976	0.0006	0.0026***
	(0.0072)	(0.0007)	(0.0016)	(0.9309)	(0.0007)	(0.0010)

Notes: Authors' estimation of their prefered version of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents (controling for birth province, birth year and EPH survey year fixed effects as well as local GDP and exposure to public administration strikes and including a cohort-specific and a province-specific linear time trend). The treatment variable has been split into 2: teacher strikes that occur in grades 1-4 and those that took place in grades 5-7. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-year. The coefficient is interpret as the effect of being exposed to teacher strikes for ten extra days during primary school. Standard errors are clustered at the birth province-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 14: Robustness and Sensitivity Checks

	Years of Education	Occupational Sorting	Log Wage	Total Earnings	Unemployed	Home Production
Panel A: Excluding city of Bs.As.						
Male	-0.0262***	-0.0015***	-0.0032***	-1.7039*	0.0008**	0.0009**
	(0.0063)	(0.0003)	(0.0010)	(0.9884)	(0.0003)	(0.0004)
Female	-0.0217***	-0.0003	-0.0019	-1.9064**	0.0009	0.0027***
	(0.0064)	(0.0006)	(0.0014)	(0.8684)	(0.0005)	(0.0008)
Panel B: Excluding province and city of Bs.As.						
Male	-0.0235***	-0.0012***	-0.0034***	-0.6997	0.0005	0.0007
	(0.0066)	(0.0003)	(0.0011)	(1.0091)	(0.0003)	(0.0005)
Female	-0.0227***	-0.0004	-0.0021	-2.1205**	0.0008	0.0027***
	(0.0067)	(0.0006)	(0.0014)	(0.8895)	(0.0006)	(0.0009)
Panel C: Excluding provinces with high migration						
Male	-0.0234***	-0.0013***	-0.0030***	-1.5504	0.0009***	0.0011**
	(0.0065)	(0.0003)	(0.0010)	(1.0053)	(0.0003)	(0.0004)
Female	-0.0228***	-0.0003	-0.0026*	-2.0903**	0.0007	0.0028***
	(0.0067)	(0.0006)	(0.0015)	(0.8582)	(0.0006)	(0.0008)
Panel D: Balanced panel (survey year greater than 2010)						
Male	-0.0216***	-0.0015***	-0.0023**	-1.8006*	0.0008**	0.0012**
	(0.0075)	(0.0004)	(0.0010)	(1.0646)	(0.0004)	(0.0005)
Female	-0.0203***	-0.0001	-0.0016	-1.3777	0.0009	0.0033***
	(0.0065)	(0.0007)	(0.0017)	(0.9980)	(0.0006)	(0.0008)

Notes: Authors' estimation using 2003-2015 EPH data on 30-to 40 year old respondents. Each column estimates the aurhors' prefered version of equation (1) unless otherwise specified (controling for birth province, birth year and EPH survey year fixed effects as well as local GDP and exposure to public administration strikes and including a cohort-specific and a province-specific linear time trend). Panel A exclude the City of Buenos Aires (CABA). Panel B excludes both CABA and the province of Buenos Aires. Panel C excludes the five provinces with the highest cross-province mobility rates (Chaco, Corrientes, Misiones, Rio Negro and Santa Cruz). Panel D eliminates pre-2010 EPH survey years to obtain a balance panel. Panel E shows results from the falsification test where we have reassigned the treatment variable for cohort c to cohort c+7. Panel F incorporates province-specific linear birth year trends to the estimation of equation (1). Panel G drops the top 1 percent of the teacher strike exposure distribution. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-year. The coefficient is interpret as the effect of being exposed to teacher strikes for ten extra days during primary school. Standard errors are clustered at the birth province-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 14: Robustness and Sensitivity Checks (Continue)

	Years of Education	Occupational Sorting	Log Wage	Total Earnings	Unemployed	Home Production
Panel E: Reassigning treatment from cohort c to cohort c+7						
Male	-0.0061	0.0006	0.0022	-1.7665	-0.0003	0.0002
	(0.0097)	(0.0004)	(0.0013)	(1.1931)	(0.0003)	(0.0005)
Female	-0.0132	-0.0011	0.0007	0.0649	-0.0002	0.0002
	(0.0095)	(0.0007)	(0.0022)	(1.0177)	(0.0005)	(0.0009)
Panel F: Including province-specific linear cohort trends						
Male	-0.0192***	-0.0017***	-0.0045***	-3.9414***	0.0007*	0.0008
	(0.0072)	(0.0004)	(0.0013)	(1.2856)	(0.0004)	(0.0005)
Female	-0.0119	0.0002	-0.0020	-2.9745***	0.0014**	0.0037***
	(0.0083)	(0.0008)	(0.0018)	(0.9408)	(0.0007)	(0.0010)
Panel G: Eliminating cohorts expose to >200 days of strikes (top 1%)						
Male	-0.0262***	-0.0015***	-0.0032***	-1.4455	0.0007**	0.0006
	(0.0063)	(0.0003)	(0.0011)	(1.0499)	(0.0003)	(0.0004)
Female	-0.0209***	-0.0002	-0.0019	-1.9827**	0.0009	0.0028***
	(0.0064)	(0.0005)	(0.0013)	(0.9098)	(0.0006)	(0.0009)

Table 15: Effect of controlling for non-teacher strikes and GDP

	Years of Education	Occupational Sorting	Log Wage	Total Earnings	Unemploy.	Home Production
Panel A: Without controls for PA strikes and GDP						
i. Male						
Strike Exposure	-0.0233***	-0.0015***	-0.0034***	-2.1796**	0.0008**	0.0006*
	(0.0060)	(0.0003)	(0.0010)	(0.8811)	(0.0003)	(0.0004)
ii. Female						
Strike Exposure	-0.0176***	-0.0003	-0.0020	-2.5964***	0.0010**	0.0029***
	(0.0058)	(0.0005)	(0.0013)	(0.8065)	(0.0005)	(0.0007)
Panel B: With controls for PA strikes and GDP						
i. Male						
Strike Exposure	-0.0262***	-0.0015***	-0.0032***	-1.7039*	0.0008**	0.0009**
	(0.0063)	(0.0003)	(0.0010)	(0.9884)	(0.0003)	(0.0004)
PA Strike Exposure	0.0004	-0.0005	-0.0014	-2.0821	-0.0001	-0.0010
	(0.0105)	(0.0006)	(0.0025)	(1.9350)	(0.0006)	(0.0007)
GDP	-1.4222***	-0.0355	-0.0345	-6.9421	-0.0020	0.0132
	(0.4128)	(0.0239)	(0.0636)	(75.9597)	(0.0247)	(0.0274)
ii. Female						
Strike Exposure	-0.0217***	-0.0003	-0.0019	-1.9064**	0.0009	0.0027***
	(0.0064)	(0.0006)	(0.0014)	(0.8684)	(0.0005)	(0.0008)
PA Strike Exposure	0.0121	-0.0005	-0.0012	-3.3382**	0.0002	0.0009
	(0.0133)	(0.0011)	(0.0025)	(1.6255)	(0.0010)	(0.0016)
GDP	-0.7139	-0.0662	-0.0531	-74.3703	-0.0049	0.0406
	(0.4515)	(0.0490)	(0.0994)	(92.1208)	(0.0291)	(0.0557)

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents. Panel A excludes controls for public administration strikes and province-specific GDP. Panel B includes these controls, both defined at the time the cohorts were in primary school. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-year. The coefficient is interpret as the effect of being exposed to teacher strikes for ten extra days during primary school. Standard errors are clustered at the birth province-year level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 16: Effect of local labor market conditions on teacher strikes, 2003-2014

	Teacher Strikes	
	(1)	(2)
Unemployment rate	0.6355**	1.1255
	(0.2591)	(0.9366)
Average wage	0.3605	-1.8366
	(0.6432)	(5.0689)
Average per capita income	0.0016*	-0.0072
	(0.0009)	(0.0061)
Days of strike in public administration		X
Province FE		X
Year FE		X
Province-specific time trends		X
R-squared	0.047	0.407

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data and strike data from CTI. The unemployment rate, average wages and average per capita family income describe the labor market conditions for each birth province-calender year cell. Column (1) regress the days of teacher strikes during the period 2003-2015 only on labor market conditions. Column (2) adds days of strikes in public administration, calendar year and province fixed effects and province-specific time trends. Regressions are weighted by the number of individual observations used to calculate the averages for province-year. Robust standard errors in parenthesis. The coefficient is interpret as the effect of local labor market conditions to days of teacher strikes. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 17: Short-Term Effects of Strike Exposure (12-17 Year Olds)

	Public Education		Years of Education		Home Production		Not Enrolled	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: All								
Strike Exposure	-0.0057***	-0.0039**	-0.0155**	-0.0179**	0.0014	0.0015*	0.0029**	0.0031**
	(0.0019)	(0.0015)	(0.0078)	(0.0077)	(0.0009)	(0.0008)	(0.0013)	(0.0013)
% Effect	-0.72%	-0.49%	-0.19%	-0.22%	2.30%	2.46%	3.65%	3.90%
Panel B: Males								
Strike Exposure	-0.0034*	-0.0020	-0.0280***	-0.0294***	0.0020**	0.0021**	0.0039**	0.0040***
	(0.0020)	(0.0017)	(0.0093)	(0.0088)	(0.0009)	(0.0009)	(0.0015)	(0.0015)
% Effect	-0.43%	-0.25%	-0.35%	-0.37%	3.29%	3.45%	4.90%	5.03%
Panel C: Females								
Strike Exposure	-0.0081***	-0.0059***	-0.0019	-0.0050	0.0007	0.0009	0.0018	0.0021*
	(0.0023)	(0.0019)	(0.0075)	(0.0076)	(0.0010)	(0.0010)	(0.0012)	(0.0012)
% Effect	-1.02%	-0.74%	-0.02%	-0.06%	1.15%	1.48%	2.26%	2.64%
Province FE	X	X	X	X	X	X	X	X
Cohort FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X
Province-Specific Time Trends	X	X	X	X	X	X	X	X
Cohort-Specific Time Trends	X	X	X	X	X	X	X	X
Controlling for exposure to PA strikes	X	X	X	X	X	X	X	X
Controlling for wage and unemployment		X		X	X		X	X
Controlling for household characteristics		X			X			X

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 12 to 17 year old respondents. Column (1) show results using individual-level data and the same controls as in our preferred baseline specification. The model used to produce the results in Column (2) incorporates local labor market variables that may influence the wealth of the family: the unemployment rate and the average wage in each province. The model underlying the results in Column (2) further includes 4 dummies of province-specific quartiles of per capita family income and 5 dummies for the maximum educational level of the head or spouse of the household (primary education or less, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary). Public education is a dummy variable equal to one if attending a public school. Home production is a dummy that equals 1 if the respondent is neither working nor studying. Standard errors are clustered at the birth province-year level. The coefficients are interpret as the effect of being exposed to teacher strikes for ten extra days during primary school. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 18: Heterogeneous Treatment Effects of Strike Exposure on Short-Term Outcomes (12-17 Year Olds)

	Male				*Female*			
	Public School	Years of Schooll	Home Prod.	Not Enrolled	Public School	Years of Schooll	Home Prod.	Not Enrolled
Panel A: Stratification by Parental Education								
At most primary education	-0.0076***	-0.0603***	0.0036**	0.0056*	-0.0097***	-0.0205	0.0006	0.0020
	(0.0023)	(0.0144)	(0.0016)	(0.0029)	(0.0023)	(0.0124)	(0.0017)	(0.0021)
Some secondary education	-0.0039**	-0.0364***	0.0023**	0.0048**	-0.0087***	-0.0063	0.0019*	0.0032**
	(0.0017)	(0.0102)	(0.0011)	(0.0019)	(0.0021)	(0.0083)	(0.0011)	(0.0013)
Secondary education	-0.0027	-0.0236**	0.0022**	0.0042***	-0.0060**	-0.0084	-0.0011	0.0001
	(0.0024)	(0.0102)	(0.0010)	(0.0016)	(0.0026)	(0.0092)	(0.0016)	(0.0018)
Some tertiary education	0.0009	-0.0220**	0.0007	0.0017	-0.0026	0.0022	0.0011	0.0026*
	(0.0025)	(0.0103)	(0.0012)	(0.0017)	(0.0028)	(0.0099)	(0.0011)	(0.0016)
Tertiary education	0.0005	-0.0114	0.0015	0.0027	-0.0048*	0.0106	0.0004	0.0013
	(0.0026)	(0.0119)	(0.0012)	(0.0019)	(0.0028)	(0.0098)	(0.0013)	(0.0016)
Panel B: Stratification by Family Income								
First quartile	-0.0082***	-0.0421***	0.0028**	0.0054***	-0.0110***	-0.0208*	0.0012	0.0027*
	(0.0029)	(0.0098)	(0.0011)	(0.0020)	(0.0027)	(0.0111)	(0.0014)	(0.0016)
Second quartile	-0.0031**	-0.0403***	0.0033***	0.0058***	-0.0089***	-0.0054	0.0014	0.0025*
	(0.0015)	(0.0103)	(0.0011)	(0.0016)	(0.0023)	(0.0073)	(0.0012)	(0.0013)
Third quartile	0.0027*	-0.0230**	0.0012	0.0025	-0.0015	0.0011	0.0008	0.0018
	(0.0016)	(0.0105)	(0.0011)	(0.0018)	(0.0018)	(0.0078)	(0.0010)	(0.0013)
Fourth quartile	-0.0015	-0.0095	0.0009	0.0020	-0.0037	0.0056	-0.0001	0.0011
	(0.0027)	(0.0102)	(0.0010)	(0.0016)	(0.0024)	(0.0086)	(0.0012)	(0.0014)

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 12 to 17 year old respondents. The results are based on individual-level regressions and the underlying model contains the same controls as that in column (2) of Table 17. Panel A interact the treatment variable with 5 dummies for the maximum educational level of the head or spouse of the household (primary education or less, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary). Panel B interacts the treatment variables with 4 dummies of province-specific quartiles of per capita family income. Standard errors are clustered at the birth province-year level. The coefficients are interpret as the effect of being exposed to teacher strikes for ten extra days during primary school. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.